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The Psychological Review

INDIVIDUAL DIFFERENCES BEFORE, DURING AND AFTER PRACTICE

BY H. L. HOLLINGWORTH
Columbia University

The literature of mental tests may be said to fall under two general headings, according as the purpose of the investigation reported has been (1) the immediate practical use of tests as instruments in the solution of some ulterior problem, or (2) the critical examination, analysis and testing of the instruments themselves. This paper belongs to the second of these groups. In the direct application of mental tests it has been too often assumed that the actual performance of an individual, in one or a dozen trials at a given task, is, in some way or other, significant of that individual's final capacity for such work. It is true that several investigators (notably Whitley and Wells) have studied the effects of practice on individual differences. These workers were interested above all in questions as to relative rate of improvement, or amount or permanence of gain. Such studies have produced suggestive results, although they have been based, for the most part, on records of only a few subjects or on a relatively small number of practice trials.

Thus Whitley concludes, " . . . the criticism that a single trial is unreliable is true but need not be exaggerated, since other facts . . . also enter in to make trials unreliable. To overcome this at least two trials should be made of any test, preferably in addition to fore-exercise in similar work. . . . The criticism that giving only a few trials measures not the mental process supposedly tested but merely adaptability to

strange conditions . . . is seldom of weight. . . . The criticism that tests measure the degree or amount of previous similar experience rather than actual capacity is true not only of such tests but of any form of mental measurement. It should operate only against expecting too much of the tests, not against their use, but rather, in fact, of repeating them at stated intervals. The only alternative,—testing subjects with no similar previous experience or else those whose training had brought them to the physiological limit—would be impracticable and out of the question. . . . The criticism that practice may influence individuals each by a law of his own, and processes each by a law of its own, does not seem to hold so far as the general law of improvement goes" (Tests for Individual Differences, 137–8).

Wells also is interested in the question of the extent to which individual differences "as we meet them in every-day laboratory experience, may be fundamental, inherent in the original nature of the individual, or may have been produced by special environment and training." His conclusions, in the case of a study of Addition and Cancellation tests, are as follows:

"We have then, finally, (1) a difference in the individual's (resp. function's) fundamental plasticity, *i. e.*, ability to profit by practice, (2) a difference in the actual amount of practice experienced, and (3) constitutional factors, independent of plasticity, in the nervous system. . . . In the present instances their influences would seem to operate in about the order named" (Relation of Practice to Individual Differences,—*Amer. Jour. Psychol.*, Jan., 1912).

The present study is concerned primarily with a combination of the problems of Whitley and Wells. To what degree are individual differences after a given number of trials indicative of the final maximal capacity of the individuals concerned? At what various rates do the factors enumerated by Wells enter into the practice curves of a group of workers, and what manner and amount of displacement in their relative abilities are thus produced? At what point or points in the curves do the individuals assume their final order of relative

capacity after training? How do the replies to these questions vary with the character of the test? By *final capacity* is here meant a degree of ability which, having been attained as the result of constant practice, remains practically unchanged by further practice during, say, 100 later trials. This seems to be a fairly correct description of what has been called, in the case of such tests, the 'physiological limit.' I shall later point out the meaninglessness of this term in such a connection.

The experiment consisted in putting each of 13 individuals through 175 repetitions of 7 different familiar tests. The trials were controlled as thoroughly as possible with respect to such incidental factors as interim occupation, exercise, food, rotation of tests, temperature, illumination, and incentive and interest. The subjects, four women and nine men, ranging from 18 to 39 years in age, were mature, and zealous, and competition was stimulated by the award of desirable prizes. Records were announced to the subjects only after each 35 trials. So far as previous practice in these particular tests is concerned, all the subjects were naïve. Five trials were made daily, these trials being distributed through the day at about two-hour intervals. The tests themselves occupied about 40 minutes at each trial (total for all subjects).

The tests were as follows:

1. Adding,—17 mentally to each of 50 two-place numbers and reciting aloud the correct answer. Order of numbers random at each trial. Record with stop watch,—time taken.

2. Naming Opposites,—correctly, of each of 50 adjectives which occurred each time in random order. Record,—time taken.

3. Color Naming,—the Columbia laboratory form of this test, with 10 repetitions of each of 10 colors. Position of card changed at each trial. Record,—time taken.

4. Discrimination Reaction,—discriminating between red and blue, and reacting with appropriate hand. Record,—sigma.

5. Cancellation,—crossing out digits from the Woodworth-Wells form of this test. Record,—time taken for 75 correct cancellations of equally difficult digits.

6. Coördination,—the familiar 'three-hole' test, for accuracy of aim. Record,—time for 100 correct strokes.

7. Tapping,—executing 400 taps at maximal speed, with hand stylus, right hand, elbow support. Record,—time taken.

Record has been here taken of the following points in the curves of practice:

1. Preliminary trial........................called initial trial
2. Median of regular trials 1–5........................5th trial
3. Median of regular trials 20–25........................25th trial
4. Median of regular trials 46–50........................50th trial
5. Median of regular trials 76–80........................80th trial
6. Median of regular trials 126–130........................130th trial
7. Median of regular trials 171–175........................175th trial

Table I

Correlations of Order of Position of Thirteen Individuals Before, During and After Practice

The correlation is in each case with the final order, after 175 practice trials (in two cases 130 trials). All coefficients are positive except where otherwise indicated.

The Test	Prelim. Trial	5th Trial	25th Trial	50th Trial	80th Trial	130th Trial	175th Trial
Adding	.154	.193	.874	.869	.973	.962	1.000
Opposites	−.088	.616	.490	.835	.945	.984	1.000
Colors	.682	.891	.858	.913	.968	.968	1.000
Discrimination	.676	.621	.604	.500	.500	.785	1.000
Cancellation	.665	.676	.885	.686	.934	1.000[1]	......
Coördination	.528	.793	.770	.902	.946	1.000[1]	......
Tapping	.231	.484	.627	.682	.693	.885	1.000
Averages	.41	.61	.73	.77	.85	.92	1.000

At all of these points the 13 subjects were arranged in order of relative ability for the test at the given stage of practice. Each of these orders, or cross sections of the group of practice curves, was correlated with the final order of position as shown in trials 170–175. Table I. gives the coefficients of correlation derived in this way, by the formula $r = 1 - \frac{6\Sigma d^2}{n(n^2 - 1)}$. A careful study of this table is instructive.

It is at once evident that the preliminary trial is in no sense a measure of the final relative capacities of the individuals tested. *Opposites*, *Adding* and *Tapping* give correlations which are practically zero, *Opposites* indeed yielding a

[1] Not included in average.

coefficient which is actually negative. *Coördination* stands considerably higher (+.53) but in no case does the correlation with the final order exceed +.68. *Color Naming*, *Cancellation*, and *Discrimination* are quite alike, giving coefficients of +.68, +.68, and +.67. In fact it is not until the 80th trial is reached that the majority of the coefficients rise to +.90 or over. Even here one is but +.50 and another +.69. Not until the 130th trial, indeed, do all the coefficients become +.80 or over. The average of all 7 coefficients increases from +.41 at the preliminary trial to +.92 at the 130th, as follows:

	Prelim.	5th	25th	50th	80th	130th	175th
Av.	+.41	+.61	+.73	+.77	+.85	+.92	+1.00

As the trials proceed then, the relative positions of the 13 individuals become more and more definitely fixed. The rate of this process however varies from test to test, and that considerably. *Adding* shows changes in position which effect a correlation of +.87 only after the 25th trial. Beyond this point there is little change, the 80th and 130th trials correlating equally well and practically perfectly (+.97) with the final order. After 25 trials, then, the final capacities of the individuals in the *Adding* test may be said to be indicated fairly accurately. *Opposites*, in the 50th trial, yields a coefficient equal to that of *Addition* in the 25th, and by the 80th trial the correlation may be said to be complete. Only after 50 trials, then, can the test be said to yield comparative measures which reflect the final capacities of the individuals in this form of controlled association.

Color Naming, *Discrimination*, *Cancellation* and *Coördination* show up to much greater advantage. Even the preliminary trials in these tests show fairly high correlations with the final orders (+.68, +.68, +.67, and +.53). With *Color Naming* this degree of correspondence increases gradually, but the 5th trial (+.89) in this test gives as good an indication of final capacity as does the 25th trial in *Adding* or the 80th in *Opposites*. *Discrimination* shows, on the contrary, a uniform decrease from +.68 in the preliminary trial to only +.50 in the 80th, and even the 130th is only slightly higher than the 1st. In the cases of *Cancellation* and *Coördination* only 130

trials were made, and the correlations are with the last 5 trials (126–130). There is, in both cases, a gradual increase as practice proceeds, in the coefficients in these two tests. In the case of *Tapping*, and quite unexpectedly to the writer, it is only at the 130th trial that the correlation with final position exceeds +.69.

In general, if we assume a coefficient of +.75 to constitute the minimum degree of correspondence with final order required for the satisfactory practical determination of the relative capacities of the members of a group, the various tests yield this coefficient at the following points:

Test	Point Where *r* is at Least +.75
Color naming	5th trial
Coördination	5th trial
Cancellation	25th trial
Adding	25th trial
Opposites	50th trial
Tapping	130th trial
Discrimination	130th trial

Or if a correlation of two thirds (+.67) instead of three fourths be taken as the minimal desirable, the points are as follows:

Test	Where *r* is at Least +.67
Color naming	1st trial
Cancellation	1st trial
Coördination	5th trial
Adding	15th trial
Opposites	50th trial
Tapping	50th trial
Discrimination	130th (also 1st, but not maintained)

Except with *Opposites*, *Coördination* and *Tapping*, a preliminary trial is as reliable as is the median of the five trials just following it. Except for *Adding* and *Tapping*, five trials after a preliminary trial give correlations of +.60 or over with the final order. Only between the 25th and the 50th trials do the average correlations reach +.75 and an average coefficient of over +.90 is not reached until the 130th trial.

The meaning of these figures seems to be that before one attempts to interpret individual differences as disclosed by performance in such a series of simple tests, he should have clearly in mind the distinction between temporary proficiency

and ultimate capacity. If he is interested, for example, in determining the vocational prospects of a youth, or the relative merits of candidates or culprits, it is important that he realize that relative abilities in many of these laboratory tests may be changed quite beyond recognition by continued work. It is highly desirable to know more than we do about the degree to which initial and intermediate trials in these tests reflect final capacity. In the past the question seems hardly to have been asked. Individual differences in early trials in some tests are fairly significant of the working level to which the performer may be brought later. In other tests this is not the case.

Indeed there is little evidence that even the final level maintained for 100 trials or more in these experiments represents what may be called, in any correct sense, a physiological limit for the individual concerned. This level may represent the 'best he can do' under the circumstances, but so did the first trial, and the second, and every other. The limit in these earlier trials was just as 'physiological' as that after 175 trials. The actual processes of articulation and movement (or of mere reading aloud) may be made much more quickly than any of these individuals have been able to perform the tests involving articulation. Each level may, indeed, constitute a 'psychological limit,'—that is, the maximal efficiency which will be attained on the basis of the present incentive. But additional incentive, such as hunger or filial devotion might change notably the relative positions of the individuals. At this point in the curve of practice only a slight absolute change of level is required to bring about such shift in relative position. And it is measurement by relative position in one's group that is most likely to be of practical consequence.

It would be of interest to determine to what degree these changes in relative ability through the medium of practice are due merely to qualitative changes in the tests. *Opposites*, for example, and *Adding* show preliminary trials which do not correlate closely with the final orders of capacity. It is probable that with some individuals these tests come, after practice, to resemble the *Color Naming* test in character. The process would then involve less and less of the element of choice or

selection and the test would tend to be performed by rather automatic association between the stimulus and the response. Such a change would account for the failure of the first trials to correlate with the last only in case the change came more quickly with some performers than with others,—with some after a few trials, with others not until after some 50 trials.

Change in the nature of the tests, variations of methods of attack, and specific improvement in the directness, independence and rapidity of the special nervous connections concerned,—these three factors would all show up in the results in the form of "changes of ability." A useful piece of work in the case of all tests will be the analysis of the changes resulting from practice. But in any case the presence of these changes in correlation shows that we are not, in early trials, measuring the same thing with all performers. The concrete tasks of daily life doubtless show just such qualitative changes, during practice, as we may suppose to be present in some of these tests. Just as it is ultimate capacity in daily life that is, with a given set of incentives, most important, so in the laboratory the measurement of 'ability after practice' ought to be more emphasized than it is at present.[1]

[1] Cf. a related article, "Correlation of Abilities as Affected by Practice," H. L. Hollingworth, *Jour. of Educ. Psychol.*, Sept., 1913.

A TIME EXPERIMENT IN PSYCHOPHYSICS.

Part II

BY DARWIN OLIVER LYON AND HENRY LANE ENO

Part I. of this article appeared in the Psychological Review for July, 1912. At the close of that paper, the authors stated that it was their intention to continue the experiment by the use of other methods—different both as to the treatment of the data, and as to the nature of the stimuli. The present article embodies not only the results thus obtained, but attempts, also, to answer and explain the various criticisms and objections that have been developed since the publication of the first article. No effort will here be made to repeat the defense of the nine 'possible sources of error' mentioned in Part I.,[1] although a few words further will be said upon electrotonus and kindred phenomena.

An entirely new apparatus was constructed, utilizing, by way of improvement, any additions or modifications that experience with the old apparatus had shown to be desirable. An additional apparatus was made, also, for the giving of tactual in the place of electrical stimuli.

Description of the New Apparatus

Although especially constructed on an entirely fresh plan, the former mechanical method of giving the two successive stimuli was retained. For a detailed description of this part of the apparatus, the reader is referred to the first article. The main changes made in the later machine were such as would assure great accuracy in its running; the new devices securing a minimum variation in the rotary speed of the disks, with the consequent ability to measure accurately the exact time interval between the various pairs of stimuli. A fair

[1] Lyon and Eno, 'A Time Experiment in Psychophysics,' Part I., Psych. Rev., Vol. XIX., pp. 326–327.

idea of this new apparatus may be obtained by a study of Figs. 1 and 2.

In the old apparatus the disks were revolved by a falling weight; in the present machine they are driven by a one fourth horse-power dynamo. The current is supplied by a large storage battery. By changing belts or shifting the controls, a wide range of velocities can be obtained. The fly-wheel weighs over 200 lbs. and by its momentum insures great regularity in the rotation of the disks. The number of revolutions per minute is read from a tachometer. Even after several hours of running, no variation of speed is observable, and in every way the actual working of this apparatus is highly satisfactory.

In order to do away with any objection or explanation of the 'seeming discrepancy' in our results, having as its basis a change in the *electrical condition* of the nerve, be it electrotonus, or what not, as well as to compare the results obtained with some stimulus other than electrical, an apparatus was constructed by which tactual stimuli could be given.

This tactual or 'hammer apparatus,' as we have called it, may be seen in Fig. 1, resting on the small table. The essential part of it consists of two small metal hammers one half inch in diameter and tapered to a point, as shown in the photograph. The hammers are controlled by small coils which, in turn, are attached to the disks in the same manner as are the electrodes that give the electric stimuli, and thus, like the electrical stimuli, the hammer strokes can be separated by any desired interval of time. By altering the amount of the current, the force of the strokes may be increased or diminished at will. As may be seen in the photographs, the coil and hammers are fastened to sliding boards set upon an adjustable arm rest.

By the experiments described in the previous article it was shown[1] that if two electric stimuli of like intensity were applied to the musculo-cutaneous nerve at points some eight inches apart,—the most convenient points being at the wrist and just below the elbow—the stimulus at the wrist (St^1) had to be given about one fortieth of a second before the stimulus

[1] *Op. cit.*, pp. 318–326.

FIG. 1.

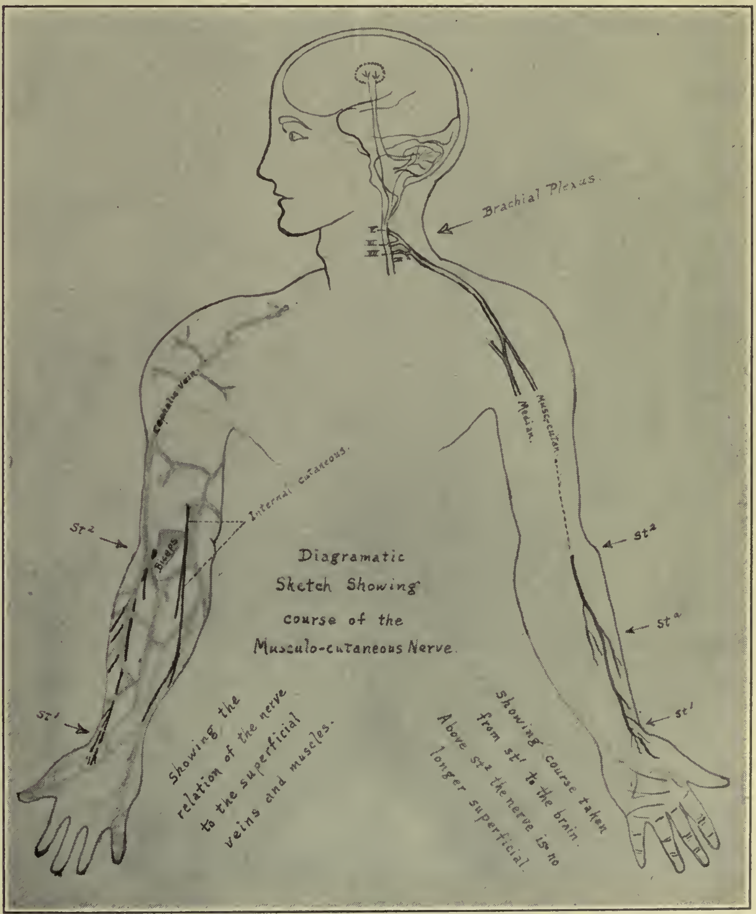
Brachial Plexus.
Cephalic vein
Musculo-cutan
Median.
Internal cutaneous.
Biceps
St^2
St^2
St^1
St^1
Diagramatic Sketch Showing course of the Musculo-cutaneous Nerve.
Showing the relation of the nerve to the superficial veins and muscles.
Showing course taken from st^1 to the brain. Above st^2 the nerve is no longer superficial.

FIG. 2.

at the elbow (St^2) if it was desired to make the two sensations appear simultaneous to the observer. If the speed of the nervous impulse, as generally accepted, was even approximately correct, this interval is fully three times as long as we should expect to find it.

The method employed was to gradually increase the interval between the stimuli until the subject felt them as separate in time, *i. e.*, one before the other. A converse method was also used whereby, starting with stimuli that were obviously non-simultaneous, the time interval was gradually shortened until the two stimuli appeared to occur together.

If the results of the preceding experiment are correct, it follows that, since St^1 must be applied 1/40 sec. before St^2 for the two stimuli to be felt together, if St^1 is applied at any *less* interval than 1/40 sec. (as 1/60 sec.), before St^2, the observer should be aware of St^2 first by a small fraction of a second, although St^1 is actually given beforehand.

In our earlier experiments as described in Part I. we used a modification of the 'method of just noticeable differences,' endeavoring by 'working from both directions'[1] to determine, as nearly as possible, the interval at which the two stimuli had to be applied apart, for them to appear together in consciousness. This interval was found to be on the average .0263—a trifle over 1/40 of a second. The method used had the advantage of speed and simplicity, but we desired to substantiate our results, if possible, by a more accurate method. What, for example, would the *majority* of subjects in the *long run* answer for stimuli given at .0333 of a second apart? We know from the experiments described that when St^1 is given .0412 sec. before St^2, St^1 is felt before St^2. The reader might justly assume that with an interval of .0400 sec. St^1 would *generally* be felt before St^2, but not always—since they *sometimes* would seem to be simultaneous at this interval. It was to discover the percentage of 'right'[2] judgments in such cases, and thus

[1] *Op. cit.*, p. 321.

[2] In one sense of the word, with an experiment like the one in question, there can be no such thing as a 'wrong' judgment. What we mean by 'wrong' is that, had the subject's attention, nervous condition, etc., been normal, he would not have made this 'out of the ordinary' judgment.

arrive at a more exact 'period of simultaneity,' that the 'method of right and wrong cases' was used.

If, after a large number of observations, a subject made no error when the stimuli were separated by a certain time interval, we would be justified in assuming that he could really distinguish one sensation before the other without guessing. By making the time interval smaller, we would naturally be justified in assuming that, although he could still, in the main, tell one sensation before the other, he might, now and then, make a 'wrong' judgment, *e. g.*, he might say that the two sensations appeared to occur at the same time. By making the time interval still smaller, so small, for example, that he was only right three fourths of the time, the subject would be noticing differences much smaller than he did when he was invariably right. Thus by setting the disks in such a way that the subject is sometimes right and sometimes wrong, we have our 'method of right and wrong cases.'

In applying this method to our experiment, we were met with certain difficulties. In the first place, we had no positive objective criterion or 'standard' to go by, for as already explained, there are no means of determining before each and every trial what the exact time interval should really be in order to have the two sensations appear synchronous.

Another difficulty is that we here have three possible contingencies: St^1 may be felt before St^2, St^1 may be felt after St^2, St^1 and St^2 may be felt together. We found, however, that the most satisfactory method was to set the disks in such a way that St^1 was given 1/60 sec. before St^2. With this interval, although the two stimuli might appear synchronous during some observations, yet the majority of observations would give St^2 as occurring before St^1.[1] With intervals as small as this, all three of the possibilities would obviously be sometimes obtained. Our method, however, was to disregard the exact nature of the answer and merely require the subject to say which of the two stimuli appeared to him to be felt first. No 'synchronous' answers were allowed, and we always

[1] When St^1 is applied 1/60 sec. before St^2, St^2 is nearly always felt first. With this interval the proportion of wrong to right results is as 1 : 6.

insisted on the subject making a *guess*, knowing that in the long run the number of right guesses would be more apt to be right than the number of wrong guesses.

Space does not permit a presentation of the data, nor is this necessary. Suffice it to say that the results obtained by this method were much the same as those obtained by the old method, viz., that in order to get St^1 and St^2 to appear together in consciousness, St^1 had to be given from 1/80 to 2/80 sec. before St^2, the average being 1/40 sec.[1]

Experiments were made with the hammer apparatus as well as the electrical apparatus, but the results in each case were identical. In both cases when St^2 is applied 1/60 sec. after St^1, St^2 is felt first, and the reaction experiments conclusively show that the nervous impulse cannot travel slowly enough to obtain such a result.

Possible Sources of Error

In the preceding article (Part I.) nine 'possible sources of error' or 'explanations of the apparent discrepancy' were discussed, explained, and dismissed to the best of the authors' ability. At the time of writing (1912), they were the only 'explanations' that had occurred to either of us. Since the publication of the article in question, three new 'possibilities' have either suggested themselves, or been called to our attention.

We also feel that 'Objection No. 9'—that referring to a possible change in the electrical condition of the nerve—was not fully answered. We therefore take this opportunity of adding a few words further on this point—after which we shall consider the three new 'possible explanations.'

In our previous article the question was raised as to whether the first stimulus might not, by causing a change akin to *electrotonus*, result in a retardation or acceleration of the propagation of the second stimulus. With the first contingency we have nothing to do, for the simple reason that St^2 is not retarded; on the contrary, it would appear to be hastened.

Strictly speaking, *electrotonus* is the modification of irrita-

[1] This is the average obtained from over five hundred tests on thirty subjects.

bility of a motor nerve caused by the passage through it of an electric current. The condition holds to a certain extent for the sensory nerves, but not to so great a degree. Though a similar condition is caused by a single induction shock, the laws of electrotonus, as they are generally formulated, presuppose a *constant* current, a factor that does not enter in our experiment. During the passage of a constant current through a nerve, the irritability of the nerve is increased in the region of the cathode, while at the anode it is diminished. The change in the nerve that gives rise to this increased irritability in the region of the cathode is spoken of as catelectrotonus, while in the region of the anode the change is known as anelectrotonus. This law, if we may so term it, remains true for each and every method of determining the changed irritability, be it a single induction-shock, an interrupted current, a mechanical or chemical stimulus, and it holds true not only for the so-called 'muscle nerve preparations,' but also for the intact nerves as they lie in the living body.

The results derived from the present series of experiments would, however, seem to eliminate the possible action of electrotonus as a source of error in this case.

We have found that St^2 is felt first, even when St^1 is given 1/60 sec. beforehand. Therefore, if this result is to be explained by electrotonus, either the nervous process set up by

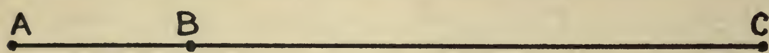

Fig. 1.

St^1 must be retarded, or that set up by St^2 accelerated, sufficiently to account for the discrepancy.

To put it graphically:

If we call the point of application St^1 *A*, that of St^2 *B*, and the cortex *C*—

1. The process from St^2 must travel from *B* to *C* in less than 1/60 sec. while the process from St^1, after taking *more* than 1/60 sec. to go one third of that distance from *A* to *B*, takes *less* than 1/60 sec. to accomplish the three times longer path from *B* to *C*, *i. e.*, it travels at not only a much slower, but also an uneven rate; or,

2. The process from St^1 must be retarded by more than 1/20 sec. before reaching *B*, and that *before* St^2 is given at all; or, if the process from St^1 is not retarded,

3. The process from St^2 must be accelerated in such a way as to overtake and pass the process from St^1 on its way between *B* and *C*.

Now it is difficult to see how electrotonus could cause any of these effects.

Condition 1 is impossible, because, we know from other reaction experiments that the process from St^1 travels from *A* to *C* in, at the most, 2/60 sec. Therefore, although it may take *less* time in transition, it cannot take more.

2. It is inconceivable that the process from St^1 can take *more* than 1/60 sec. to travel over the free path from *A* to *B*, and less than 1/60 sec. to go from *B* to *C*—a path involving the brachial plexus, medulla, thalamus, and possibly parts of the cortex itself.

3. It is also improbable that one nervous process starting after another nervous process, and travelling in the same path, can overtake the first process and pass it, without some kind of fusion.

The most conclusive proof, however, that neither *electrotonus*, nor any other kindred electrical phenomenon, lies at the root of the explanation, is that the same results are obtained with the hammer apparatus, which gives tactual stimuli only.

Of the further possible sources of error, the first[1] that we shall consider is the possibility that nervous impulses from the two points of stimulation end at different cortical 'levels' instead of proceeding to the same place; in short, that the impulse given by St^1 involves a greater distance—be it anatomical or physiological—than the impulse from St^2. Graphically this explanation might be shown thus

Fig. 2.

where S^1 and S^2 refer to the corresponding sensory areas.

[1] As a possible explanation this criticism was brought to our attention by Dr. Head, editor of *Brain*, and by Professor H. C. Warren, of Princeton University.

In answer to this objection we would say that what little is known of the anatomy of the sensory tracts shows no indication of any such arragnement. Not only have we no reason for assuming any such anatomical state of affairs—but, run to the extreme, such an assumption would mean that the greater the distance away from the brain at which a stimulus is given, the shorter is its path in the brain itself.

It is true that we still know comparatively little of the course of the sensory neurones after they leave the tegmentum and optic thalamus, but we have no reason for thinking that the centripetal impulses started at or near the periphery of a cutaneous nerve change in character as they travel inwards. It should be remembered that the possible paths for the conduction of afferent impulses are many, and that they become more and more complex as the various tracts approach the brain. It is thought that the greater part of the sensory impulses reach the optic thalamus, and from here are distributed to the various parts of the cortex, but of the exact anatomical arrangement we know almost nothing. The localization area of sensory impressions in the cortex is thought to be posterior to the motor area—but even of this we have no definite knowledge. All we know is that the sensory area seems to be less sharply defined than the motor area in that it occupies the greater part of the parietal lobe as well as the posterior central convolutions. Seeing how unsatisfactory is our knowledge with regard to the sensory tracts in the relatively simple spinal cord, and how little we know of their distribution between the medulla and the thalamus, it is obviously useless to attempt to form any anatomical explanation of the 'apparent discrepancy' when we know practically nothing at all of the final distribution of the tracts in question. All we can say is that the little which embryology can tell us points against any such explanation.

Moreover, a brief reference to the well substantiated results of reaction-time experiments will show that no such difference in time between the sensations corresponding to two stimuli, such as we are considering, can be due to the traversing of respectively longer and shorter paths in the

brain by the nerves involved. For if an increasing difference of 1/40 second occurs for every 8 inches in the length of a nervous path, as the points of stimulation are taken further from the brain, a reaction time from the neck should, roughly speaking, occur in a time equal to the reaction time from the hand less a proportionate time, at the rate of 1/40 sec. for each 8 inches of the additional distance to be traversed. Thus for the extra 32 inches, it would be 1/10 sec. less 4/40 sec. That any such explanation is impossible is evident when we calculate the reaction time of the neck on this basis—which would be 1/10 − 1/10 or 0.

In like manner a reaction time of the foot would consume an altogether disproportionate interval, since a touch reaction time rarely rises about 2/10 second. But, as it is well known, there are no parts of the body which, when stimulated, show reaction times which are either abnormally long or nearly instantaneous.

Furthermore, if a stimulus be given at the shoulder with one of the hammers, the other hammer can be seen to move *before* the sensation at the shoulder is felt, when the hammers are timed in such a way that the hammer at the shoulder strikes 1/60 sec. before the hammer which is visually perceived only. Therefore, if a brain process and its correlative sensation were simultaneous, the nervous impulse from the shoulder must take at least this 1/60 sec., plus the unknown time (X) which the visual impulse consumes, to reach its appropriate sensory area.

Now if a further time of 1/40 sec. for each 8 inches is added as the stimulus is given further down the arm, it would take 1/60 sec. plus, as before, the time X, to which must be further added 3/40 sec. (for the additional 24 inches from shoulder to hand), or almost 1/10 sec. in all for a nervous impulse from the hand to reach the cortex.

But a *reaction time* from the hand[1] can occur in 1/10 sec.; hence there is only 1/10 − 1/10 or 0 seconds left, in which the

[1] The hand-to-hand reaction time includes whatever time is required for the transmission of the efferent as well as the afferent nervous impulse, together with muscle innervation, etc., which, of course, makes the comparison even more absurd.

association processes, efferent impulse, and muscular innervation must all take place—which is palpably absurd.

Furthermore, that no such difference as 1/10 sec. exists between the sensations from the hand and eye is easily proved by direct experiment, for if the hammers are timed so that the strokes occur 1/10 sec. apart, the touch of the first hammer is distinctly and invariably felt, by all observers, before the second hammer is seen to move.[1]

It would seem, therefore, that there cannot possibly exist any such retardation of the nervous impulse from St^1 due to its proceeding to some endpoint involving an additional cortical path beyond the endpoint reached by the nervous impulse from St^2.

Another objection raised is that, in an experiment where a discrimination between two successive sensations is involved, additional 'perceptual' centers may also come into play, as p^2 and p^1 in the diagram below.

FIG. 3.

These perceptual centers would, further, be connected by an associational path (x), and the longer time taken for St^1 to get itself perceived in comparison with St^2 might depend on the additional cortical area, which must be brought into action before the process set up by the nervous impulse from St^1 emerges into consciousness.

In answer to this objection, it is only necessary to point out that it makes no difference *what parts of the cortex or brain* the impulses from St^1 and St^2 eventually reach and involve as the correlative substrata of their respective sensations or perceptions.

The considerations indicated in the answer to the first objection show that it is impossible that any such discrepancy in time between the perceptions of the two stimuli could be taken up through the mere implication of different areas.

[1] The 'smallest interval' between sight and touch is rarely more than 1/20 sec. Ladd & Woodworth, 'Psychology,' p. 475.

For any discriminative reaction from the two stimulated points in question necessarily involves those same perceptual areas, and a discriminative reaction can be gotten in .18 sec. which would be impossible if the conditions urged in this objection obtained; nor could the time interval between stimuli from two different points, as from the two hands, where both hemispheres of the brain are involved, be distinguished, as it is well known to be, at such minute fractions of a second as 1/360.

The involving of different areas as the essential concomitants of these two sensations or perceptions, or both, if they can account for the time difference in question, must do so either because the specific character of the area affected by St^1, or the mere fact that *different* areas are involved, always retards the sensation of the stimulus which is given first.

We have seen that a longer path does not furnish an adequate explanation. Nor can the cerebral effect of the first stimulus always be retarded; for, in that event if St^2 should be given first by any interval (say 1/60 sec.) sufficient to overcome the time taken by the nervous impulse to travel from the points St^1 to St^2—in that event St^2 should be felt first. But, as we have shown, this is not the case.

A further objection should also be mentioned briefly.[1] It is the general criticism that our present knowledge of what actually happens in the brain is so vague, that we know so little what areas are involved as substrata of any given mental process, that conclusions drawn from the localization of definite sensory or perceptual tracts or the nature of any particular processes, must necessarily be untrustworthy.

As we have seen, however, in the discussion of the preceding objection, neither exact localization nor accurate description of process are essential for the validity of our experimental results. They hold good whatever areas may be involved, or of whatever nature the cerebral processes may, ultimately, be found to consist.

[1] This objection was called to the authors' attention by Dr. Wm. McDougall, of Oxford, and by Professor Henri Bergson.

Summary

All the possible physiological causes that might explain the different relative times which our experiments show to be consumed by the nervous impulses from a stimulus at the wrist (St^1) and a stimulus at the elbow (St^2) in reaching and setting up the appropriate cerebral processes correlative to their respective sensations must fall under one of two headings, either:

1. The impulse from St^1 must be retarded, or
2. The impulse from St^2 must be accelerated.

This acceleration or retardation, also, must be due, either

(*a*) To the physiological conditions at the point of stimulation, nerve path, or brain area involved, in each case, or,

(*b*) To the fact that St^1 is set back because it is given first in time, or St^2 set forward because it is given after St^1.

In the preceding article and the present paper, we have pointed out that there seem to be not only no anatomical or physiological grounds of explanation, but that it can be shown experimentally, by a comparison with known reaction times, that no such retardation or acceleration can exist.

It has been demonstrated by actual experiment, also, that whichever stimulus is given first in time, the results are not materially altered.

Conclusion

In a series of experiments extending over two years, and carried on by means of various apparatus and methods which, as far as the authors can ascertain, have thoroughly substantiated the results, it has been discovered that when two successive stimuli, either electrical or tactual, are applied at different points over the same nerve, there exists an altogether disproportionate interval between the respective times of stimulation and the occurrence of the correlative sensations.

The two points of stimulation chosen for the majority of the experiments were upon the musculo-cutaneous nerve at the wrist and just below the elbow. These points are, in general, about eight inches apart, but the time elapsing between the application of the stimulus at the wrist and its corre-

sponding sensation is some 1/40 sec. longer than the elapsed time between stimulus and sensation when the stimulus is given at the elbow.

As the velocity of the nervous impulse is at least thirty meters a sec. and, therefore, cannot take more than 1/20 sec. to traverse the eight inches between the points of stimulation, the unaccountable discrepancy between the times of occurrence of the corresponding respective sensations is approximately 1/60 sec.

In searching for an explanation for this discrepancy, we have examined in detail, in the present and preceding articles, various possible sources of error which can be roughly grouped under the following headings:

A. Anatomical. Under this heading are included:
  1. Difference in the sensitivity of the skin at the two points of stimulation.
  2. Difference in depth of nerve at the two points.
  3. Number of synapses and nerves involved.
  4. Different perception centers.

B. Physiological.
  1. Fusion of successive impulses in any portion of the nervous system—central or peripheral.
  2. Frequency with which the two nerve tracts involved are accustomed to transmit stimuli.
  3. Difference in velocity in different parts of the neurone.
  4. Electrotonus and kindred phenomena.

C. Psychological.
  1. Inaccuracy of observation.
  2. Effect of attention on any part of the neurone, including the so-called 'hair-trigger' condition, monopoly of the subject's attention by the first stimulus, etc.

As a result of the examination of the above possible sources of error, we have been unable to find that they adequately explain so great a discrepancy in the time intervals between the two stimuli in question and their respective correlative sensations. Since, however, the experiment would appear to indicate that, when the two stimuli are so timed that the corre-

sponding sensations occur simultaneously, the correlative cortical processes *do not* occur simultaneously, it would seem to follow that, in the case of sensation, at any rate, the cortical and psychic processes are not *synchronous;* but that the cortical process precedes its correlative psychic process by a small, but not experimentally imperceptible, interval of time.

THE EFFECT ON FOVEAL VISION OF BRIGHT SURROUNDINGS—II

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The material of the present paper is a continuation of work recently reported.[1] The apparatus, the technique of procedure and the method of computation of results are identical, so details will be treated here only in so far as they vary from those of the previous work.

One of the two observers is the same for both pieces of work. Since the two alternated as experimenter and observer, it is natural that the technique of procedure would be modified, in a certain incalculable way, by the replacement of the second individual. Every essential detail, however, was kept as nearly as possible the same as it was before.

The only essential variation in conditions was the substitution of surroundings for the test object of brightness 2.87 candles per square meter, in place of surroundings of brightness 41.9 candles per square meter, which is approximately of 1/15 the brightness formerly used. Further, instead of completing the observations with dark surroundings before beginning those with bright surroundings, as was done before, the two were carried out over the same period of time. This manner of procedure is obviously preferable to the other, but was not carried out before owing to delay in the construction of the apparatus used to make the surroundings.

Incidental Observations

Reference was made in the previous communication to certain disturbances experienced when the test objects were highly illuminated and observed in dark surroundings. Observer *J* in these experiments experienced some disturbance under the same conditions. At intensity *a* there was a glare

[1] Psychological Review, XX., pp. 425–447.

always disagreeable and sometimes painful, the outlines of the object often becoming blurred. This disturbance was greatly decreased under bright surroundings. At intensity *c* under bright surroundings the conditions were exceptionally pleasant. There was some discomfort at intensity *e* with

TABLE III

Brightness of Test Object			Surroundings Dark							Surroundings, 2.87 Candles per Sq. Meter						
Designation	Candles per Sq. Meter	Log of Same	Relative Visual Acuity	Mean Var. P. C.	Visual Angle Minutes	Diff. Limen Per Cent.	Mean Var. P. C.	Mixed Region Per Cent.	Mean Var. P. C.	Relative Visual Acuity	Mean Var. P. C.	Visual Angle Minutes	Diff. Limen Per Cent.	Mean Var. P. C.	Mixed Region Per Cent.	Mean Var. P. C.
a	83.35	1.921	5.36	1½	0.50	0.26	86	1.71	27	5.27	2	0.51	0.42	65	1.29	35
			3.87	6	0.70	0.50	52	1.34	34	3.95	4	0.68	0.50	58	1.49	25
b	14.8	1.171	4.89	2	0.55	0.16	126	1.21	60	5.08	4	0.53	0.32	75	0.94	37
			3.52	4	0.77	0.30	81	1.38	48	3.65	6	0.74	0.51	51	1.13	39
c	2.90	0.462	4.26	2	0.63	0.35	78	1.09	50	4.33	2	0.62	0.25	84	0.80	53
			3.24	6	0.83	0.21	78	.99	40	3.18	6	0.85	0.26	93	0.80	37
d	0.593	9.773	3.58	4	0.75	0.15	124	1.05	46	3.69	6	0.73	0.59	99	1.43	76
			2.77	4	0.97	0.48	72	1.54	50	2.59	5	1.04	1.21	40	2.92	26
e	0.176	9.244	2.89	4	0.94	0.31	117	1.45	49	2.55	4	1.06	1.39	64	4.23	52
			2.36	6	1.14	1.21	76	1.88	81	2.06	5	1.31	4.04	34	6.67	36

The upper figures in each case give the results for observer *J*, the lower for *C*.

bright surroundings. The apparent brightness of the respective halves of the field, as well as the lines on the acuity object, would frequently appear to shift during exposure of the test object. Judgments at this intensity were more uncertain than at any other.

The results are given in Table III. and embodied in Figs. 7 to 11 where, as before, the abscissæ (brightness of the test object) are in every case plotted as logarithms, and the ordinates as the actual difference per cent. or as minutes visual angle in the case of visual acuity.

INDIVIDUAL DIFFERENCES BETWEEN THE OBSERVERS

The present results show that in vision both at absolutely low brightness of the test-object and also where the latter was low only relatively to the surroundings, *J* has better discrimination than *C*, the difference being quantitatively about

the same as the difference shown between *G* and *C* in the preceding work, although the curves in the present case do not so readily admit of a numerical estimation of this difference. Further, it appears that at all points, with a few exceptions, *J* showed better discrimination than *C*. This is most clearly evident in the case of visual acuity (Fig. 9) where *J* shows a

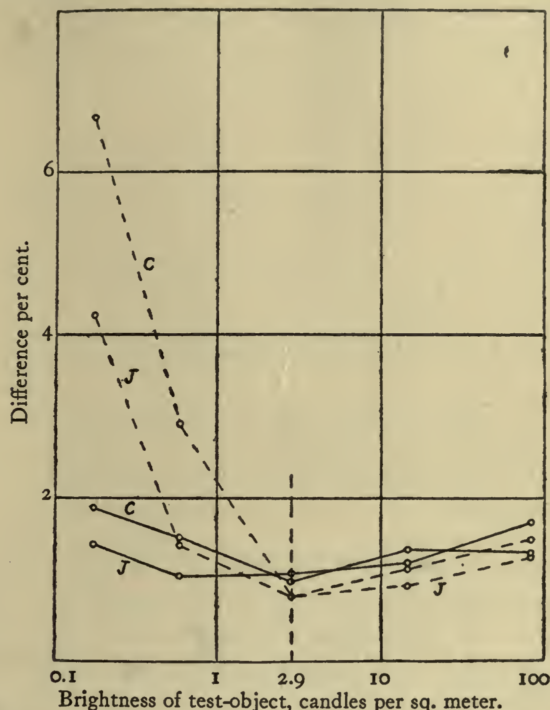

Fig. 7. Extent of the Region of Mixed Judgments (*M*-values) for the Two Observers under Various Conditions. Dark surroundings ————————. Surroundings 2.9 candles per sq. meter – – – – – – – – – – –.

better value by a ratio of about 7 to 5 throughout. In the case of the *M* values (Fig. 7) and the difference limen (Fig. 8) there are a few exceptions, but on the whole the difference is seen from the curves to be a general one.

Differences Due to the Surroundings

As before the bright surroundings are seen in all cases to cause more or less extreme loss in visual discrimination when

the test object is observed at a brightness much less than that of the surroundings. At the point of equality and in the cases where the test object is seen in surroundings less bright than itself, the results are by no means so decisive. For instance in Fig. 8 the difference-limen at *a* and *b* (test object 83.3 and 14.8 candles per square meter) is for both observers greater with surroundings of 2.9 candles per square meter,

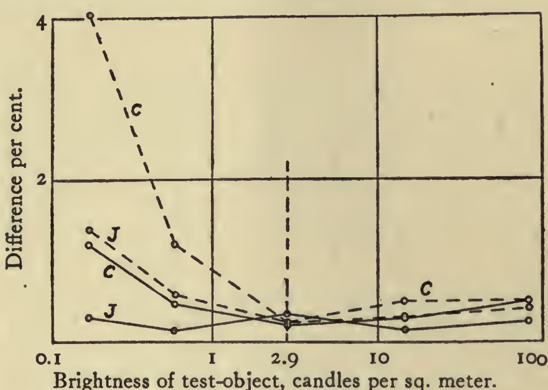

FIG. 8. Difference-limen (*L*-values) for the Two Observers under Various Conditions. Dark surroundings ————————. Surroundings 2.9 candles per sq. meter – – – – – – – – – – –.

than in dark surroundings, while the test object when observed at a brightness equal to these surroundings gives contradictory results with the respective observers. It is not easy to see why this should be, for if surroundings equal in brightness to the test object give results (at least) as good as those obtained with dark surroundings, it is hard to see why surroundings of brightness greater than zero and less than that of the test object should bring about inferior discrimination.

As to the *M* values (Fig. 7) these indicate greater consistency of judgment wherever the test object is of brightness equal to or greater than that of the surroundings than in the corresponding cases of dark surroundings. The one point to be noted as an exception is that of the highest brightness of test object in the case of observer *C*, where the result is just the reverse, and is subject to the same remark as that made in the last paragraph in discussing the difference-limen.

Visual acuity (Fig. 9) exhibits certain irregularities but the general course of the curves, and the comparatively small mean variations in the individual results, justify a somewhat briefer and more general conclusion from them, namely that bright surroundings which are not brighter than the test object itself result in slightly better vision than the dark surroundings. Surroundings which are bright much in excess of the test object give results less marked in the case of visual acuity than in the case of brightness difference, but neverthe-

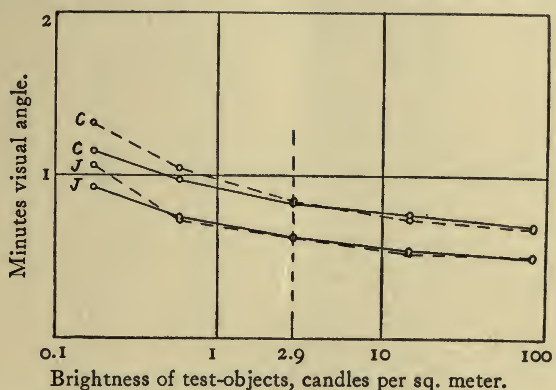

FIG. 9. Least Visual Angle (*V*-values) for the Two Observers under Various Conditions. Dark surroundings ——————. Surroundings 2.9 candles per sq. meter – – – – – – – – – – –.

less, plainly following the general rule that excessively bright surroundings interfere seriously with vision.

The mean results for both observers, *C* and *J*, are given graphically in Fig. 10. Fig. 11 is a summary of the difference limen and least visual angle for all three backgrounds; dark, 2.87 and 41.9 candles per square meter. The results of observer *C* only are used here since they are the only ones comparable under the three conditions.

Discussion

Comparison of the visual acuity and brightness-difference curves under parallel conditions show that as the test-object brightness is reduced the difference-limen usually at a fairly definite point takes rather an abrupt rise. Visual acuity, on the other hand, while always showing a slight progressive

diminution beginning at the very highest brightness under a similar change of conditions, never undergoes such rapid decrease as differential sensitivity. This fact is to be considered in connection with the almost obvious fact that discrimination of fine detail depends upon (*a*) a physically perfect image on the retina and (*b*) probably upon the accurate fixity

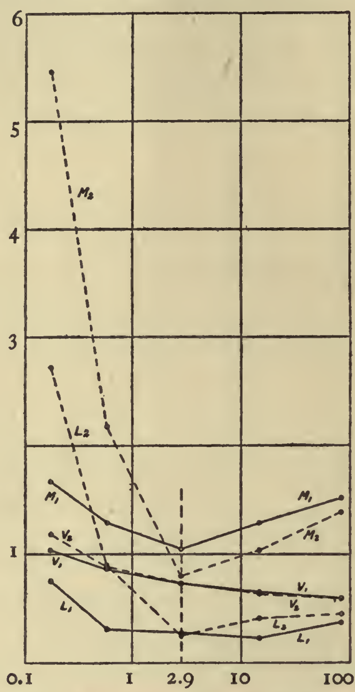

FIG. 10. Mean Values of *M*, *L* and *V* under Various Conditions. The ordinates represent difference per cent. in the case of *M* and *L* and minutes visual angle in the case of *V*. Dark surroundings ————————. Surroundings 2.9 candles per sq. meter – – – – – – – – – – – –.

of this image which in turn depends on the steadiness of the extra-ocular muscles. Since there is nothing in uniformly bright or dark surroundings to influence either of these factors (except as noted farther on) it may be concluded that visual acuity depends mainly upon these, and hence varies less under the influence of contrast than does differential sensitivity because the retinal image is always equally perfect.

The exception to be noted to this last assumption refers to the effect of the size of the pupillary aperture on the sharpness of the image. The usual assumption on this point is that a smaller pupil causes a sharper retinal image and hence a better value for visual acuity. There are several considerations of pure physical optics which enter into this question, one of which speaks in a direction exactly contrary to the usual view

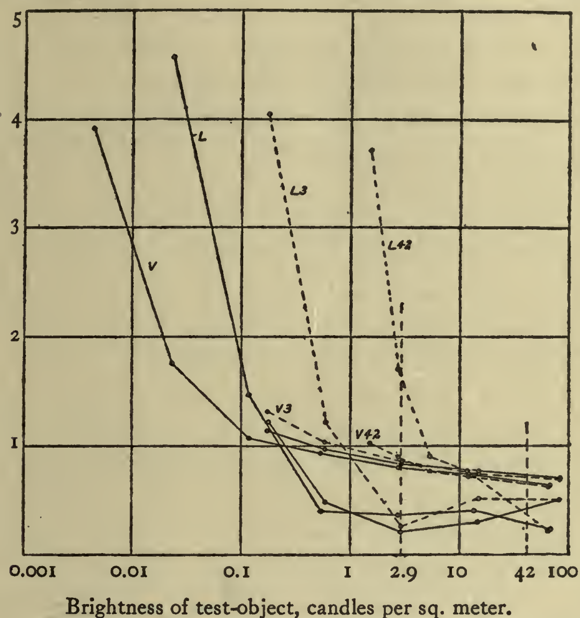

Fig. 11. All Values for L and V for One Observer (C). The ordinates represent difference per cent. in the case of the limen and minutes visual angle in the case of visual acuity. The figures following the letters designating the curves drawn in broken lines give the approximate brightness in candles per square meter of the surroundings used. The short curves drawn in solid lines represent the values for dark surroundings obtained in connection with the work of the present paper, the long ones those of the preceding work.

and a presentation of certain results is contemplated for the near future which show that under some conditions at least, a small pupillary aperture may result in inferior vision. Obviously without actual pupillary measurements the effect of the size of pupil cannot in any case be estimated.

As contrasted with visual acuity, differential sensitivity can be said to depend mainly upon retinal conditions, and to a

very minor degree upon the perfectness of the retinal image. The two forms of discrimination might be considered to be special cases of the same phenomenon rather than as different phenomena, since each involves perception of differences of both intensity and extensity. In the vision of fine detail, as well as in the perception of small brightness differences between simultaneously presented fields, there must be perceived differences in brightness which must at the same time be referred to areas differently localized. Otherwise no judgment could be made. The essential difference is that in the case of visual acuity estimation the brightness difference of the parts of the test object is gross, while the areas involved are minimal. Hence a slight mixing up of various parts of the image by dioptric irregularities of any sort produces a decided fall in visual acuity. Disturbing the refraction of a normal eye to the extent of one half diopter by the use of a convex lens brings about a decided decrease in visual acuity readily demonstrable by means of the Snellen test-letters.

On the other hand, in the estimation of differential sensitivity by simultaneous presentation the areas of the fields compared are gross while the brightness-difference is minimal. Small irregularities in the formation of the retinal image need not be expected to have any noteworthy effect in this case but the important factors would be those influencing the light sensitivity of the retina, namely the contrast-effect of the surroundings of the test-object, and the after-effect of previous stimulation of the retina, its state of adaptation and the presence or absence of after-images. The first named influence, that of the surroundings, is shown by the results to be far more significant for differential-sensitivity than for visual acuity. Investigation of the after-effects of previous stimulation of the eye was not contemplated in this work.

The explanation of the differences in visual capacity which attended the difference in surroundings as used in the present work must be referred in the first place to contrast. The most marked effect noted is what has been called by Abney the 'extinction' of light by the illuminated retina, where the surroundings are much brighter than the test-field. In the

opposite case, where the test-field is about equal to, or brighter than the surroundings the differences in vision due to these surroundings as against dark surroundings appear to be on the whole in the direction of better vision, although the smallness of the difference, the large mean variations in the case of brightness-difference estimations, the non-agreement of the results and certain factors in the technique used lay the results open to some doubt.

Aside from contrast there is another factor which undoubtedly plays a part in the depression of vision under very bright surroundings. The eye-media are not perfectly clear and every object within the visual field sends light into the eye, all of which except a probably minor fraction constitutes the retinal image. This small fraction is scattered within the eyeball. Further, the light constituting the image must undergo lateral diffusion within the retinal and subjacent tissues. This diffusion is probably far more extensive than the limits of what is known as irradiation. Just what the magnitude of illumination may be, due to the light from any particular object thus diffused to any part of the retina is not known, but it is abundantly demonstrable by the simple experiment of placing a light source somewhat eccentrically in a rather dark visual field, a few feet from the eye, and alternately shading and exposing the eye. It will be noted that when the eye is protected from the direct light of the lamp that almost the entire visual field is clearer as to detail and is darker, and that when the direct light is again admitted to the eye the field becomes at once brighter and more confused.

It is to be remembered that surfaces, such as white and black paper when seen under equal illumination, present brightnesses to the eye which are about as 20 to 1. In the two extreme cases of the present work the test-object was seen upon grounds of about 16 and 28 times its own brightness respectively. Neither ordinary vision of opaque objects by reflected light nor the conditions of the experiment just mentioned bear direct visual evidence of scattered light within the eye, since under these circumstances its effect is at least fully compensated by contrast, which acts in exactly the

opposite sense. Yet scattered light is there and its consideration is necessary to the understanding of visual phenomena.

Summary

The conclusions from the results just given are in general not substantially different from those stated in the preceding paper, except as qualified by the following statement which is to be taken in connection with conclusion (4) of the former paper:

Surroundings of a brightness about equal to or less than that of the test object show no consistently better or worse results than dark surroundings with the identical test object. On the whole, visual acuity under these circumstances was slightly improved and the difference-limen increased, but in the latter case the diffusion of the results (*M*-value) was distinctly diminished.

By a somewhat different method now under consideration it is hoped to eliminate some of the uncertainties inherent in the present one and obtain more definite results as to brightness-discrimination where the present results are unsatisfactory.

The writer wishes to acknowledge the coöperation of Dr. H. M. Johnson as experimenter and observer in this work.

THE EXPRESSION OF THE EMOTIONS

BY ANTOINETTE M. FELEKY

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It is the purpose of this article to illustrate the expressive movements characteristic of certain emotional states, or rather, to show what emotional states certain facial expressions do signify.

Several hundred photographs of the same individual, A. F., were taken at different times during a period of one year. As she posed for each photograph, A. F. had clearly in mind what she was endeavoring to portray, either by deliberately calling up the emotion itself, or by reciting words expressing the desired emotion.

Eighty-six of these photographs were presented to one hundred reliable persons. Each photograph was numbered, and each subject received also three sheets of paper. Upon one sheet were numbers which corresponded to the numbers upon the photographs, and upon the other two sheets were merely a fairly complete list of names of emotions. This list of names was as follows:

Laughter
Smiling
Joy
Delight
Pleasure
Gladness
Glee
Happiness
Amusement
Bliss
Ecstasy
Cheerfulness
Rapture
Enthusiasm
Merriment
Astonishment
Amazement

Desire
Earnestness
Eagerness
Reverence
Religious
Friendliness
Pride
Haughtiness
Self-assertion
Calculation
Meditation
Reflection
Thought
Hate
Disgust
Contempt
Scorn

Wonder
Admiration
Surprise
Awe
Attention
Interest
Expectancy
Want of interest
Modesty
Humility
Self-abasement
Grief
Sorrow
Sadness
Despair
Mental suffering
Physical suffering
Pain
Displeasure
Annoyance
Irritation
Worry
Bore
Bitterness
Hardness
Love { Religious
Altruistic
Romantic
Sensuous }
Pity
Sympathy
Vanity
Coquetry
Coyness
Liking
Tenderness
Longing
Yearning

Sneering
Loathing
Repugnance
Dislike
Aversion
Disdain
Antipathy
Rage
Fury
Anger
Distraction
Passion
Calmness
Resignation
Beauty
Ugliness
Fear
Terror
Horror
Suspicion
Dread
Alarm
Fright
Anxiety
Hopelessness
Despondency
Awe
Dismay
Timidity
Defiance
Determination
Firmness
Faith
Trust
Resolution
Aspiration
Relief
Hope

The following were the directions which the subjects received.

"*Experiment in Judgment of Expression.* Materials: Photographs, a list of words, and a list of numbers.

"1. Read through quickly the list of words in order to refresh your memory with the names of the different expressions. Observe each photograph carefully and write upon the sheet, opposite the corresponding number, the name of the expression which the photograph suggests to you. If one word

does not suffice to express the meaning, add the necessary words."

The subject was also requested to write down his introspection.

I present the facts in the case of twenty-four of the photographs. These facts are (1) reproductions of the photographs themselves, (2) a statement of the stimulus which led to the pose in each case, and (3) a statement of tabular form of the interpretations of each photograph by the hundred judges. This last statement is not complete, the names which occurred only once in the 2,400 judgments being omitted for brevity's sake. Their inclusion would make no appreciable difference, as they are few and are distributed between synonyms of an appropriate term and 'errors' in much the same proportions as the judgments recorded here.

The results give (1) means of deciding how far certain defined facial expressions are interpreted each as the sign of a given emotion or complex of emotions; (2) and, in cases where the facial expressions are clearly significant, means of studying emotional expressions and illustrating them before classes in psychology or dramatic art.

The photographs with their identification numbers are given in Plates 1, 2, 3 and 4.[1]

The stimuli to the poses were as follows:

3 was posed for the second line in Gretchen's speech to Faust:

"I feel it, you but spare my ignorance
To shame me, sir, you stoop thus low."

9 was posed for breathless interest.

11 was posed for attention to a purely intellectual matter. While mentally multiplying 19 × 19 the subject was photographed.

15 was posed for attention to an object.

18 was posed for suspicion. (This is more often interpreted by the hundred judges as *fear*. We may note that Bell describes fear and suspicion together. "In human fear and suspicion, the nostril is inflated, and the eye has that backward, jealous and timid character. . . ."

21 was posed for interest toward a child.

[1] The author will be glad to furnish series of the original photographs or half-tone reproductions thereof to anyone wishing to use them for research or instruction in psychology. The cost is not yet ascertained, but will probably not be over five cents apiece for the reproductions and twenty-five cents apiece for the originals in sets of twenty-four.

22 was posed for agreeable surprise.
29 was posed for pity ('Poor thing').
31 was posed for determination.
32 was posed for righteous anger.
33 was posed for horror.
38 was posed for physical pain.
44 was posed for fear, the exposure being made after the subject had said the word 'Poison' in reciting Juliet's speech in the potion scene—"What if it be a posion, which the friar subtly hath ministered, to have me dead?"
47 was posed for hate.
48 was posed for sympathy.
50 was posed for despair.
51 was posed for rage.
52 was posed for vanity.
55 was posed for disgust.
61 was posed for sneering.
62 was posed for contempt.
69 was posed for laughter.
77 was posed for religious feeling.
83 was posed for the first degree of suspicion.

The interpretations by the judges, omitting terms occurring only once in the 2,400 judgments, are given in Table I. Where the same photograph is described by one judge by two terms (*e. g.*, hatred and scorn), each is counted as one half. By following down any column, the interpretation of any one of the photographs may be seen clearly. For example, Photograph 61 has 93 judgments in the disgust, repugnance, sneering, scorn, contempt group, 1 of bitterness, 1 of defence, 1 of hate, 4 of the terms applied to it being omitted because occurring only once in the entire 2,400. By reading the table horizontally one sees what different expressions may be accepted as significant of 'modesty,' 'coyness,' 'fear,' etc. For example, coyness and coquetry, mentioned 66 times, are applied 16 times to No. 3, 16 times to No. 29, 28 times to No. 52, twice to Nos. 21 and 83, and once to Nos. 18 and 62.

It must be kept in mind that a part of the variation in the judgments of the same photograph is due to ignorance of the meanings of real facial expressions and to ignorance of the accepted meanings of the terms used.

It is hoped that the very considerable success of these posed photographs will lead others to publish snap-shots of men, women and children in naturally aroused emotional states. The last should be relatively easy to obtain.

Table I

Photograph	3	9	11	15	18	21	22	29	31	32	33	38	44	47	48	50	51	52	55	61	62	69	77	83
Modesty	22½		1			3		1																
Coyness	10					2		11										9						1
Coquetry	6				1			5										19			1			1
Contentment	2																						1	
Earnestness	2½					1	1																	
Reproach	1							1																
Shyness	7																							
Uncertainty	1			½																				
Yearning	2																						1	
Disgust					2						½	2		12½	1				36	8	6			1
Repugnance				1	5		1						1	5	2		1		14	1				1
Dislike			1		1				1	4				2	5				7	2	3			1
Annoyance			1	1	3⅓					3		1	1		4		4		8					
Loathing					2									2					4½	2½				1
Aversion			½	1	6						1		½	5	1				2	2				4
Bored			2						1	1		1		1	1		5		2	1	4			
Sneering														2½					8	33	10½			
Scorn					1½				2					7	2		1		3	16	11			3
Contempt									1	1				2½			1		3	19½	21			2
Disdain	1													8			1		1	7	21			1
Haughtiness																				1	9½			
Superiority						1															2			
Snippy, superciliousness																					2			
Cynicism			1																		1			
Horror		2			3						35		9½		½	1	16½							
Terror					1						13		14			5								
Alarm					2½					3½	3		6			6½	2½							3
Fury											9			1			3							
Frenzy											1						1½							
Fright					2				1		7		12			2½	½							
Passion																	2							

TABLE I.—*Continued*

Photograph	3	9	11	15	18	21	22	29	31	32	33	38	44	47	48	50	51	52	55	61	62	69	77	83
Suffering, physical					1						5	31½			3		3							
Suffering												3					½							
Suffering, mental			1						1	1		17	2	1	5	5	1							
Worry			1		2				3	8		8½	2		10	5			1					1
Anguish										1				2										
Despondent										1		2½				2								
Grief					1							1	2		1	3								
Pain											2	6	1	1		1	1		1					
Trouble										1		1												
Sorrow				1										½	3½									
Suspicion	2				6			1		2			1	1			1							31½
Antipathy					1					1			1	1	1⅓						1			2½
Distrust					½																			6
Jealousy									1															1½
Nothing			1							1	1													2
Timidity	1½					1								1										7
Watching				1																				2½
Doubt	1	1	1							1														1
Self-abasement					1																			1
Laughter									3													40		
Ecstasy																		3				11	1	
Glee																		1				17		
Merriment																						10		
Mirth																						2		
Rapture		1					1											2				6½		
Reverence																							25	
Religious																							15	
Religious love, faith, etc.																							25	
Aspiration		1																					1	

TABLE I.—*Continued*

Photograph	3	9	11	15	18	21	22	29	31	32	33	38	44	47	48	50	51	52	55	61	62	69	77	83
Adoration																		1					3	
Devotion																							4	
Humility	3																						3	
Prayer																							6	
Resignation			2																				2	
Suppliant																							2	
Trust				1														½					1	
Fear					18⅓					1	4		8½		2⅓	2								5
Anxiety			1	2	5					7		6	6		9½	4								
Dread					9					1	1	2½	5½	1	3	1	2							
Expectation of disaster					1					1														
Interest	4	1	15	22	1	23½	½			1								4						1
Eagerness			1	5	1																			
Enthusiasm		1		2			1															1		
Expectation		5	4	18½		2				2½			1					1						
Hope			2	5½			1			2													2½	
Want of interest	1		1	4	1	1						1			1	1								
Amusement	1					8½		½										2				2		
Cheerfulness			1			2																		
Friendliness						4½		2										2						
Liking	1			1		5		1																
Meditation	1		2			4						1				1								
Romantic love				1		2	1																1	2
Vanity						1		1										10			2			
Beauty																		5						
Bliss																		4						
Delight		1																5						
Gladness								1										2						
Happiness																		4				1		

TABLE I.—*Continued*

Photograph	3	9	11	15	18	21	22	29	31	32	33	38	44	47	48	50	51	52	55	61	62	69	77	83
Love								1										6						
Pleasure			1			2		½										8						
Playful							½	1										1						
Smiling				1		2												3						
Self-satisfaction																		2						
Desire		1		1												1		1						
Tenderness	2			1		10		18										1						
Sympathy	5		1	½		7½		14½		1			1		9½									
Pity	3			1		½		10						½	5									
Comforting a child								2																
Cajoling	2							4																
Chiding								2																
Loveal truistic, maternal						2		2																
Pleading								2																
Persuasion								2																
Soothing capability								2																
Attention	2	5	15	13		4½				2			1		½	1								
Calmness	1		9																					
Calculation			3	1		1			1	2					4						1			1½
Curiosity			1																					½
Reflection	1		5	1			½														1		1	1
Sadness	1		2																					
Deep, penetrating thought			2						1															
Amazement		13					9			2	4½		1			1	3							
Astonishment		13					10½	½		2	½		2½			2	1½							
Surprise		30				2	52	½		2	2		2½			2	2½					½		
Wonder		14	1	2½			12					1	1½			2								
Awe		6	1				2			2	1	1	2			1							1	
Admiration		4		1		1	3											1						

PLATE I.

3

9

11

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PLATE II.

22

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PLATE III.

44

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52

PLATE IV.

55

61

62

69

77

83

TABLE I.—*Continued.*

Photograph	3	9	11	15	18	21	22	29	31	32	33	38	44	47	48	50	51	52	55	61	62	69	77	83
Determination			2						23½								½							
Assertion of will power									1								1		1					
Bitterness					3				5	1		2		2½						1				
Displeasure			1	½	2				8	4			1	1	5				4		1			
Defiance					3				4	1	1			5⅓			2½			1	1			2
Disapproval									2															
Firmness									8½	½							½							1
Hardness									3	2			1	2½	1		½							
Resolution									6	2														
Self-assertion									1					1										
Sternness									3															
Despair					½											13	1							1
Dismay					1		2			4	1½	1	3		1	8	4							
Distraction					1					1		1				10	1							
Bewilderment																2								
Hopelessness												1			1½	1		½						
Perplexity										2		1			1	3								
Racking brains																1½								
Thought	3		4	½						4						5								1
Anger					1				7½	9	1½		8	1⅓			9							1½
Hate					1½				1					8			3			1				
Hatred									½										1					
Indignation									2	1							2				1			
Dissatisfaction										1							1							
Hurt			1							1														
Irritation			1		1				1	4				2	5				3					
Rage					4				1		4		1				9							
Puzzled										2														
Ugliness														13										

A SLIT-MECHANISM FOR SELECTING THREE MEASURABLE MONOCHROMATIC BANDS

BY H. M. JOHNSON

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The device herein described was designed for the purpose of selecting a single band of spectral light, which is to be matched in hue by mixing two other bands. For demonstration-experiments it is desirable, and in precision-experiments necessary, that the wave-length of the stimulus-bands be accurately measurable. As the writer's purpose made the use of a single spectrometer system desirable, the method of selecting the bands presented some difficulties. It was necessary to obtain a spectrum of maximal purity and intensity: hence it was inadvisable to use a long slit in the collimator-tube or to use an objective lens of great focal length to increase the height of the spectrum. For this reason it was decided to select the three bands from the same horizontal plane.

Abney[1] devised a set of three slits, moved independently in a groove in which they fit. Each slit opening is made by two jaws which form the long sides of a parallelogram whose short sides are two pieces each of which is pivoted to a projection in the center-line of the slit-opening and fastened at either end to one of the jaws. A screw-and-spring mechanism regulates the width of the slit-opening. The method of selecting a given band is as follows: The width of a given slit-opening is found by comparison with a standard slit. The slit is then placed in the groove and moved by hand to the desired place. This is indicated on a transparent scale, calibrated in terms of wave-length, which is fastened above the groove. An image of this scale 10 times magnified is thrown on a screen; from which readings can be made to .02 mm. This device, though ingenious, and from Abney's account satisfactory, was not adopted because of its manifest clumsiness.

[1] Abney, Wm. de W., Researches in Color-vision, etc., N. Y., Longmans, 1913.

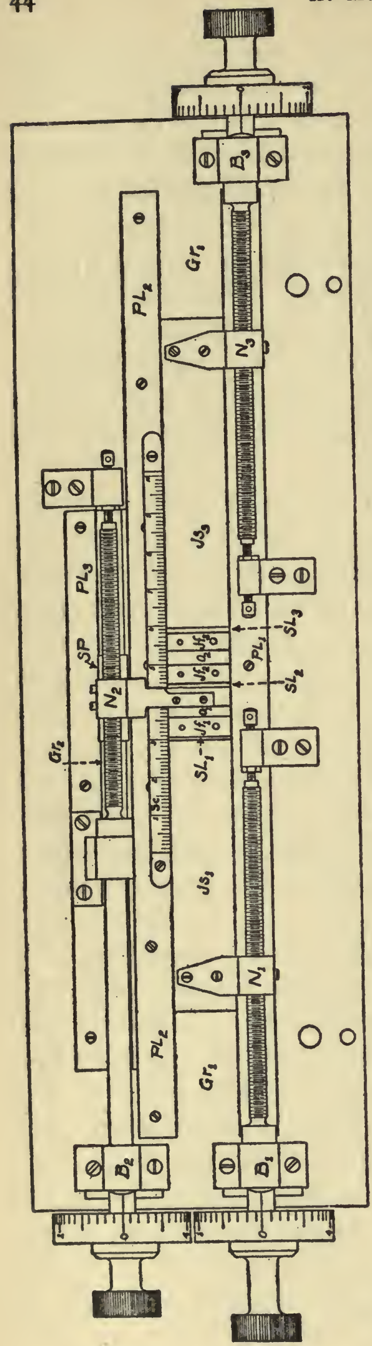
The double slit designed by Watson[1] and built by Gaertner and an earlier model of similar type—the Donders coupled slits, suggested this design in essential. The work was executed by Mr. William Würth, the mechanician of this laboratory, at a cost of about $90.00. It was found possible to make the wall and the shafts of the micrometer screws considerably shorter than in the Donders and the Watson models, and to use a single scale for the coarse readings. These changes tend toward compactness and simplicity without disadvantage.

The accompanying figure shows the construction in detail. All the parts are of brass except the shafts, nuts and bearings of the micrometer screws and the millimeter scale, which are of steel.

In the slit-wall a window is cut 1 cm. high and 9 cm. long. Its vertical limits are visible through the three slit-openings Sl_1, Sl_2, and Sl_3 and the intervening spaces O_1 and O_2 in the cut. Each slit-opening is formed by two knife-edged jaws, one free (Jf_1, Jf_2, Jf_3) and one (Js_1, Js_2, Js_3) attached to a split nut (N_1, N_2, N_3) working on a micrometer screw. All the jaws are beveled to fit accurately into a common groove, Gr_1, in which they slide. This groove is formed by two parallel beveled plates, Pl_1 and Pl_2, fastened by screws to the slit-wall above and below the slit-window, respectively. The free jaws Jf_1, Jf_2, Jf_3, are held in place by spring clips fastened to the center of the jaw on the reverse side and binding against the beveled edges of the window.

The greatest mechanical difficulty was the prevention of a serious amount of lost motion in jaw Js_2, occasioned by its narrowness and the great distance between its screw-shaft and the groove Gr_1. This problem was solved by attaching the nut N_2 to a long strip of brass beveled to slide in a groove Gr_2, planed parallel to groove Gr_1, and like the latter, formed by beveling two brass plates Pl_2 and Pl_3. By this means the knife-edge of the jaw Js_2 is held as truly normal to its groove Gr_1 as are the other jaws. For the suggestion which led to the adoption of this device the writer is indebted to Dr. P. W. Cobb.

[1] Yerkes, R. M. and Watson, John B., 'Methods of Studying Vision in Animals,' Behavior Monographs, Vol. 1, No. 2, N. Y., Henry Holt, 1911.



The three micrometer screw-shafts are threaded 0.5 mm. to the revolution. Each of the 50 divisions on the screw-head therefore reads 0.01 mm. If all the settings are made by moving the screw in the same direction, the readings are accurate to limits within this order of magnitude. In making a given setting, say for slit Sl_1, the free jaw Jf_1 is pushed to its proper place by the jaw Js_1, the latter is now withdrawn beyond its proper place and returned. The reading to 0.5 mm. is made on the scale *Sc* from the edge of the jaw; the finer readings are made on the head of the micrometer screw.

The greatest proximity of the two extreme bands which this device allows is 2 cm. plus an amount dependent on the width of the slit-openings. In the spectrometer system to which it is attached at present, the beam emerging from the collimator is passed through two carbon disulphide prisms and brought to a focus on the slit window by an objective lens of 15½″ focal length. With this arrangement, the prisms being set at the angle of minimum deviation for the sodium line, the linear separation of the red lithium line ($\lambda = .670\mu$) and

the sodium line ($\lambda = .589\mu$) is about 15 mm. The linear separation of the sodium line from the green strontium line ($\lambda = .548\mu$) is about 24 mm. Sufficient separation may be obtained in another way: by using a single dispersion prism with an objective lens of great focal length. Such a lens however is very expensive if it be accurately ground, and the loss of radiation occasioned by its use is greater than that made by the second prism, since the height as well as the length of the spectral slit-image is magnified.

The bands selected by this mechanism may be deflected to their projection lenses by right-angled prisms.

The apparatus can also be used as a single or double slit when required. When it is made a permanent part of a spectrometer system the scale should be calibrated in terms of wave-length. The method of calibration, which is quite simple, is described by both authors cited above. Selection of the bands desired can thus be made quickly and accurately.

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PSYCHOLOGY AS A SCIENCE OF BEHAVIOR

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To those who have grown suspicious of the definitions and methods commonly employed in psychology it is a most hopeful sign that this suspicion has gained active and vigorous support among psychologists themselves. There is evidence at present of a pronounced disposition to pause for a consideration of fundamentals. What is psychology anyway,—what is its subject-matter and what are its methods? The stock definition that it is concerned with 'the description and explanation of states of consciousness as such,' states of consciousness being something which everybody knows and nobody can define, has fallen or is falling into disrepute. Yet the assumptions involved in this definition and the procedure based on it have persisted. Criticism seems to have had no appreciable effect. Now, however, comes a challenge which cannot be ignored so lightly. This challenge comes from the *sanctum sanctorum* of the laboratory itself. It declares that the conceptions which prevail in psychology are inept for laboratory purposes. Introspection is a broken reed. All that is significant in psychology is retained and provided for if we regard psychology, not as the science of mental facts through the medium of introspection, but as a study of behavior.

This is the contention advanced by Professor Watson in a recent number of this REVIEW.[1] He charges roundly that "human psychology has failed to make good its claim as a natural science" (p. 176), adducing as evidence the futilities which pass at present as scientific psychology and the impossibility of terminating disputes concerning facts which are inaccessible to experiment and derive their warrant wholly from an esoteric method known as introspection. "I firmly believe that two hundred years from now, unless the intro-

[1] 'Psychology as the Behaviorist views it,' March, 1913, pp. 158–177.

spective method is discarded, psychology will still be divided on the question as to whether auditory sensations have the quality of 'extension,' whether intensity is an attribute which can be applied to color, whether there is a difference in 'texture' between image and sensation and upon many hundreds of others of like character" (p. 164). There is but one remedy for all this, viz., to change our problem. "What we need to do is to start work upon psychology, making *behavior*, not *consciousness*, the objective point of our attack" (pp. 175–6).

The point of view thus briefly indicated meets with considerable approval from Professor Angell,[1] who protests, however, that the indictment is too sweeping. While the procedure of the behaviorist is undeniably objective and scientific, it is at the same time subject to serious limitations. To confine ourselves to the study of behavior may be quite in place as long as our subject is a rat in a labyrinth or a young beaver in a third floor apartment. And it may be admitted, further, that the study of behavior is of great significance in human psychology. But it is also true that "what happens between the time a stimulus affects a peripheral organ and the later time at which some reaction is made, we can often only judge with approximate accuracy provided the individual concerned tells us what has passed in his mind during the interim. The same thing is true of those reactions which are made in seeming independence of any immediate sensorial excitation. In other words, we have not at present any technique for ascertaining the train of neural units intermediate between a specific sensorial stimulation and a specific delayed response. This gap we must bridge over with information gleaned from essentially introspective sources or else leave it open" (p. 266).

A further limitation of behaviorism lies in the fact that it arbitrarily excludes from psychology an interesting and legitimate field of investigation. There are persons "to whom mental process as mental process is the only fascinating and ultimately worthy subject of study." To leave out mental process is for such persons, to omit Hamlet from the plot. "To recognize and describe the *external expressions* of love,

[1] 'Behavior as a Category of Psychology,' this REVIEW, July, 1913, pp. 255–270.

hate, and anger is as different from the actual experience of these thrilling emotions and from the description of them as immediately felt, as is the inspection of a good meal from the consumption of the same. To such an one any abandonment of introspection must seem a pitiful and mean desertion of the real objects of worth. Whether this view permanently prevails or becomes an esoteric scientific cult, it is a safe prediction that we shall always have it with us'' (p. 269).

The behaviorist's program, then, according to Professor Angell is inadequate, first, because it is frequently unable to trace out the behavior of the organism without appeal to the mental processes which are going on simultaneously, and, secondly, because the rejection of mental processes as worthy objects of study is an unwarranted proceeding. Introspection still has its rights. "Let us then bid the movement toward objective methods and objective description God-speed, but let us also counsel it to forego the excesses of youth" (p. 270).

It is worthy of note that the charges brought against introspection are by no means controverted in Professor Angell's article. " 'Tis true 'tis pity, and pity 'tis 'tis true." The defense consists mainly in showing that the behaviorist had better not be throwing stones so recklessly, since he is clearly in need of an ally. Theoretically, indeed, the attempt to get at the facts from the outside, through the medium of behavior, can go a long way, but practically it encounters difficulties early and often. To achieve without the aid of introspection an extended analysis of color experiences into simple qualities, or to ascertain the peculiarities and scope of perception, memory, and imagination is abstractly possible, no doubt, but as a matter of fact, such work must necessarily be schematic and crude. How are we to learn the peculiar *modus operandi* of memory processes, unless the subject reports facts such as the presence of visual or auditory images? The purely subjective facts of which introspection puts us in possession are valuable, both for their intrinsic interest and for their service as clues to behavior. Introspection, therefore, is too valuable a tool to be lightly thrown away.

There is room for the suspicion that this line of defense

combines two things which in the interests of clearness should be kept apart. It is, first of all, a plea for introspection. Professor Angell declines to "embark on the troubled waters of definition. Suffice it to say that, however introspection be defined and whatever merits and defects may be alleged to attach to it as a method for ascertaining facts, all, so far as I know, are agreed that we are directly cognizant of our own experience in a manner different from our indirect apprehension of the experience of others. Whatever this *direct* mode of approach may involve under final analysis, it may serve for the moment to represent the sort of thing I have in mind by introspection" (p. 268, note). So much for introspection. But at the same time it is clearly taken for granted that the things revealed by introspection are in the first instance 'purely subjective facts.' Hence the assumption is made that if the behaviorist finds it necessary or expedient to use facts obtained by this 'direct mode of approach,' he ceases, so far forth, to be a behaviorist and returns once more to the wallow of subjectivism from which he extricated himself so recently and with so much pain and effort.

The plausibility of the argument lies, it would seem, in the illicit union of the 'direct mode of approach,' here called introspection, with subjectivism. In order to put an end to the scandal, the two parties to the union must be forced either to dwell apart or else to live openly before all men in holy wedlock. The behaviorist is in a position to view both alternatives with equanimity. The fact that he makes use of the direct mode of approach to obtain data convicts him of disloyalty to the concept of behavior only if such approach be taken as equivalent to subjectivism. But why should it be so taken? It would be just as reasonable to charge an exceptionally keen-eyed scientist with being an introspectionist because he observes facts which his less gifted colleagues can reach only in a round-about way. Nay more, since introspection is identified with inspection, every mouth is stopped and all the world has become guilty before the tribunal of the introspectionist. Thus interpreted, however, introspection ceases to be significant as a distinctive method. The reproach—or

the glory—of the introspective method is that it deals with a unique subject-matter, by virtue of which fact it is esoteric in character. This is the substance of the accusation which is brought against it. The hypothetical scientist just alluded to would not suppose that he was making use of any distinct method, simply because he could see what others were unable to see. Nor is it obvious why the behaviorist who selects as his problem the behavior of our scientist in making this observation is any more of an introspectionist if he consults his subject in the gathering of his data.

It would seem, then, that if introspection is to mean simply a 'direct mode of approach' and nothing more, our question disappears. It is not the mode of approach but the assumed nature of the subject matter that has made objective verification impossible. Unless we postulate a distinct subject-matter, we have simply returned to the non-reflective and naive use of consciousness; and as Professor Watson says, "in this sense consciousness may be said to be the instrument or tool with which all scientists work" (p. 176).

If, however, we take the second alternative and assume that introspection has a subject-matter all its own, the cause of introspection profits quite as little by Professor Angell's argument. Granted that the study of the external expressions is not a study of the experience itself, what do we gain if we appeal to the industry which is taking to itself the vestments of an 'esoteric scientific cult'? Is it a description of the experience to say, in the language of Professor Watson, "this, as a whole, consists of gray sensation number 350, of such and such extent, occurring in conjunction with the sensation of cold of a certain intensity; one of pressure of a certain intensity and extent, and so on *ad infinitum*"? (p. 168). If the description of thrilling emotions be the goal, the dime novel can beat psychology at its own game. Professor Angell's defense of introspection suffers from the serious handicap that he has permitted the case of his client to go by default. A counter-attack on the prosecuting attorney, together with a eulogy on honesty and sobriety, can scarcely be held to prove that his client is innocent of the charge of being a rank impostor.

So far, then, the upshot of the matter seems to be, on the one hand, that behaviorism, while incontestably scientific, is not exactly psychology, and on the other hand, that the study of 'subjective facts' or 'mental states,' while it may be entitled to the name of psychology, is neither scientific nor descriptive. If we insist on science, we must take up the study of behavior; if we crave description, our best course is in the direction of the literature of fiction. Meanwhile psychology, such as it is, remains with us. Professor Angell takes for granted that we must recognize the existence of 'mental terms,' accessible only through introspection. That there are such mental facts or 'pure psychics,' Professor Watson does not undertake to deny. "I confess I do not know. The plans which I most favor for psychology lead practically to the ignoring of consciousness in the sense that that term is used by psychologists to-day. I have virtually denied that this realm of psychics is open to experimental investigation" (p. 175). There may be mental facts, and if so, they constitute a legitimate subject for study. Moreover this study can invoke etymology in behalf of its claim to the name of psychology; and it can cite history to prove that it is the legitimate descendant of what has previously passed under that name. The worthless character of the claimant, however well established, does not warrant the usurpation of his family name and title by a stranger. If the behaviorist permits his opponent to maintain such claims, his proper course, it seems, would be to evacuate the premises and set up an independent establishment.

Such an arrangement, unfortunately, promises no lasting peace. Sooner or later the novelist will discover that he is in possession of a field which has hitherto been neglected by the sciences, and we may then anticipate that the novelist who writes like a psychologist will claim that he is really a psychologist who writes like a novelist. And he will point to the thrills in his emotions as evidence that he, and not the anemic devotee of structuralism, is the real psychologist. There is no alternative, then, but to go through the musty records and establish, if we can, the identity of the heir apparent, beyond further cavil. Who or what are these rival claimants, psychism and

behaviorism? Professor Angell admits the difficulty of defining the psychic, at least by implication; while Professor Watson states that he does not 'wish to go further into the problem [of psychics] at present because it leads inevitably over into metaphysics' (p. 175). The metaphysician, naturally pleased to find that he has a mission in life, can scarcely be blamed if he construes this indirect recognition as an invitation to state his views.

The situation is complicated by the fact that the advocates of structuralism would doubtless assert our statement of the issue to be an artificial simplification of the case and hence merely a begging of the question. Would they admit that their subject-matter is the psychic, in the sense of an existence different in kind or 'texture,' so to speak, from material objects? Their utterances on this point leave room for doubt. While there is much talk of consciousness, mental states, and psychic processes, it is also contended—for example, by Professor Titchener[1]—that the subject-matter of the psychologist is the same as that of the physical sciences. "It is the same experience all through; physics and psychology deal with the same stuff, the same material; the sciences are separated simply—and sufficiently—by their point of view." Experience taken in its independent aspect is physics and chemistry; taken in its aspect of dependence on the body it is psychology.

This view seems to place itself beyond the reach of criticisms directed against the hypothesis of mental states or 'consciousness as such.' Psychology and physics do not deal with different materials. It is the standpoint, not the stuff or subject-matter, that differentiates them from each other. But what constitutes dependence on the body is not made very clear. My own labors on this point lead me to the conclusion that the word dependence conceals an ambiguity which makes it possible to interpret consciousness in terms of behavior or in terms of mental states, as occasion may require.

To illustrate the standpoint of psychology, Professor Titchener cites the difference between physical and psychological time. Physical time is constant, psychological time is

[1] 'A Text-book of Psychology,' Chapter I.

not. 'The hour that you spend in the waiting-room of a village station and the hour that you spend in watching an amusing play are physically equal; they measure alike in units of 1 sec. To you, the one hour goes slowly, the other quickly; they are not equal' (p. 7). Time from the one standpoint is subject-matter for physics; time from the other standpoint is subject-matter for psychology.

But what, more precisely, is the nature of this difference? One would hardly care to account for the interminable boredom of waiting for a train in an out-of-the-way place by saying that a psychical or apparent time comes into being and adds itself to the physical time and thus brings about the peculiar length of the wait. The time which I experience and which taxes my endurance is the only time there is. Nor can I hope to ascertain the 'real' length of that time by consulting some person with ideal fortitude of character in order to learn how long the time seems to him. His time is psychological time quite as much as mine. The only conclusion, then, which we can draw is that physical time is a certain measurement of this duration, a rendering of it in terms of another duration—swings of a pendulum, for example—in order to get an equivalence. It all depends on the kind of inquiry that I make concerning the time in question.

To this, if I interpret him correctly, Professor Titchener would agree. It should be noted, however, that unless we go beyond this point, the distinction between physical and psychological time in terms of dependence on the body is wholly inept. So far everything that has been introduced involves dependence on the body. The swings of the pendulum are as much an experiential fact as the boredom of waiting. The distinction, in other words, becomes a distinction in the kind of problem that we treat, with no pertinent reference whatever to dependence on the body. The physical problem has to do with mathematics and equivalences; the psychological problem concerns itself with factors such as attention, habit, preoccupation, etc. The duration is studied by the psychologist in relation to the activities of the organism, not in relation to a consciousness or with reference to its 'mental' constituents.

Dependence on the body means that the psychologist studies the time with reference to the behavior of the experiencing organism.

It is not long, however, before we come upon what seems to be a second meaning of the word dependence, and one which abundantly justifies the term. "Heat is a dance of molecules; light is a wave-motion of the ether; sound is a wave-motion of the air. The world of physics, in which these types of experience are considered as independent of the experiencing person, is neither warm nor cold, neither dark nor light, neither silent nor noisy. It is only when the experiences are considered as dependent upon some person that we have warmth and cold, blacks and whites and colors and grays, tones and hisses and thuds. And these things are subject-matter of psychology" (p. 8). Again we find the statement, "It is when heat-waves strike the skin, and sound-waves strike the ear, and light-waves strike the eye, that we have experience in its dependent aspect, as warmth and tone and color" (p. 10).

Statements such as the foregoing are no doubt open to more than one interpretation. In saying that the world of physics is neither warm nor cold, the writer may have meant simply that the physicist is not interested in these qualities, but chooses to confine himself to a study of motions. The relation of stimulus to sense-organ becomes prominent only when we turn to physiology and psychology. But if that is what is meant, dependence on the body is no criterion for the distinction between physics and psychology. The body is as much concerned in motions as it is in colors. We seem to meet in these passages what is in effect the old-time distinction between primary and secondary qualities. The distinction between physical and psychological is, it seems, no longer a distinction of problems; it has become a distinction of existence or order of being. With the advent of the experiencing organism, warmth and tone and color spring into being and add themselves to the sum-total of the things already existent in the universe. These qualities become dependent upon nervous processes in a manner that does not obtain in the case of motions. Dependence is now a dependence upon processes in the nervous system which characterizes some facts as con-

trasted with others. Thus it is stated that "our sorrow is the mental aspect of those nervous changes that make us cry: we have only to shift our point of view, and what appeared as nervous change appears as emotion" (p. 15).

As already indicated, the point that I wish to emphasize is that we must come to terms as to what is meant by dependence upon body. Taking the phrase in one sense, we find that it is simply a name for the difference in problems with which physics and psychology respectively are concerned. But taken in this sense the phrase becomes a misnomer, and suggests the propriety of reading off all our psychological facts in terms of behavior. Taking it in another sense, the phrase reinstates the distinction between primary and secondary qualities, in complete disregard of what history and logic have to say on this important subject. Taking it in the first sense we get a view of psychology which apparently turns it in the direction of a study of behavior. Taking it in the second sense we get nowhere at all. All experiences being dependent in the same sense upon nervous processes, psychology must necessarily oscillate, in much the same way as has sometimes happened to sociology, between the view that it is the only science and the view that it is merely a blanket term for all science, with no specific field or problem of its own. The plausibility of Professor Titchener's position, I am forced to believe, lies in taking both interpretations of dependence at once. In this way it is possible for him, on the one hand, to maintain connections with our common world and claim that the subject-matter of psychology is the same as that of physics. But on the other hand, he is also in a position to continue the tradition of a psychology which has, after all, an independent subject-matter and an esoteric method.

The elimination of ambiguity and the repudiation of mental states, however, does not justify the view that psychology is a study of behavior. Behavior is a wide term. Professor Angell suggests that "mental life, conscious process, as our psychologists have dealt with it, has had to do with reactions which were mainly concerned with new individualistic adaptations. The behavior which we should study in man would be,

in part, therefore, the old instinctive behavior, but in part this new personalisticly adaptive behavior" (p. 262). And he adds that "as a program this is entirely intelligible."

That this program is intelligible it would not be worth while at present to dispute. Whether it is valuable as a guide to investigation is a different question. It is not at all obvious why we need a new science to study this behavior, unless we find that this behavior is truly different in kind. What is the difference between instinctive behavior and 'personalisticly adaptive behavior'? From the standpoint of evolutionary history and inherited structure there is doubtless an important difference, but the distinctiveness of psychology must be based on a difference in the behavior itself, if the definition is to justify itself. The fact, however, that a given behavior is personalisticly adaptive behavior is not, apparently, revealed in the behavior as such. It is an extraneous character, and so can give no distinctiveness to the field of psychology. It would be quite as reasonable to subdivide the field of botany in the interests of a new science, and group together for separate botanical study those flowers which have enabled poets to give symbolic expression to the beauty of women.

The first difficulty, then, which we encounter is that of differentiating the behavior which is subject-matter for psychology from other forms of behavior. A further apparent difficulty springs from the fact that behavior presupposes relation to a stimulus. In the simpler forms of behavior stimulus and response may be correlated without practical difficulty. But when we deal with what Professor Watson elsewhere calls 'delayed overt response,' the matter becomes more complicated and the theoretical difficulty becomes more prominent. The behaviorist would not seriously undertake to record everything that happens between stimulus and response. He proceeds selectively, taking the relation of stimulus and response as his clue. He is properly interested in the movements which result from the application of the stimulus only in so far as they constitute response. Otherwise his study is not a study of behavior, but a study of movements. But when does a movement constitute a response? Do we

label as stimulus the spoken word which results in overt action a week later, or the visual perception which sets a complicated and long-drawn-out problem, for no other reason than that it appears somewhere as an antecedent in the causal chain of events? If so, there is no obvious reason why the event which occurred just before or immediately after the *soi-disant* stimulus should not be regarded as the true stimulus. Unless a satisfactory reason is forthcoming, it would seem better to substitute cause and effect for stimulus and response and to drop the term behavior from our vocabulary. Psychology then becomes a study of certain causal relationships, but is still without a principle for the selection of those causal events which are supposed to constitute its peculiar subject-matter.

Even if we manage to become reconciled to this situation, however, our troubles are not yet at an end. There still remains the difficulty in certain cases of showing that the event which is selected as stimulus or cause bears any significant relationship to the event which figures in our scheme as the response. The stimulus is supposed to have a causal connection with the response, but how are we to know that this is the fact? How are we to know that the engineer who solves a problem for me at my request might not have done so anyway? No behaviorist can possibly show that the airwaves set in motion by my vocalization were an indispensable stimulus. We doubtless believe that the spoken word was in fact the spark which lit the fuse and finally exploded the mine, but this belief involves a complication of causes which it is wholly beyond our power to control or to verify.

It is true, of course, that we are able, as a matter of fact, to correlate stimulus and response. I know that it was the spoken order which caused the commission to be executed, for the expert reminds me of the fact and presents a bill. But neither of us makes any pretense that his belief is derived from a scrutiny of the causal sequence. Memory furnishes us with a short-cut to the result. While our present acts are doubtless connected with the past through causation, we do not regard them as simply the effects of antecedent causes. They are rather responses to present stimuli. The expert presents his

bill, being moved thereto by a stimulus which may be indicated by saying that it is the spoken-word-constituting-a-commission-now-completed. That is, the stimulus cannot be pushed back and anchored at a fixed point in the past, but is a present factor at the moment of response.

The point that I wish to make is that if psychology is to be regarded as a study of behavior, we are bound to reinterpret the category of behavior. We are inevitably forced to consider once again the difference between acts that are instinctive or purely automatic and conscious acts. If we attempt to account for this difference by the doctrine that sensations or some other simon-pure psychic existences come into being, we raise more difficulties than we solve. This doctrine seems as unnecessary as it is incoherent, since it is possible to assign to psychology a type of behavior which is different in kind from other behavior. A purely instinctive response to a light-stimulus may properly be viewed as response to ether-vibration or wave-length. But if this stimulus results in what is commonly called consciousness, a different kind of response ensues. The light-stimulus becomes a cause or occasion for the act of looking. But why look, unless it be to secure a new stimulus for further response? We stop to look, precisely because the first response does not run smoothly off the reel. The response will not go forward, so to speak, but is halted and expends itself in the effort to secure a further stimulus. We have here a highly peculiar form of response, in that it is a response which seeks and maintains the stimulus necessary for further response.

We reach the same result if we say that conscious response is a process of organizing or readjusting different simultaneous responses which interfere with one another. Hence the pause during which the organism prepares for the final adaptive response in which the conflicting partial responses are harmonized. This is the moment of attention, of looking, which furnishes the organism with a visual object by which the further behavior is controlled. The stimulus and the response during this period of hesitation are correlative in the sense that the process of establishing a harmonizing adjustment for the conflicting responses is paralleled in the process by which

the visual object finally attains the status of an adequate stimulus.

From this standpoint the characteristic trait of conscious behavior lies in the fact that stimulus and response develop concomitantly. As long as the response is uncertain, the stimulus is likewise uncertain. The response which involves a series of steps has as its correlate a stimulus which provides for its own successor. In a response to a visual stimulus, for example, the stimulus proves to be a stimulus, not only for various further acts, but also, more specifically, for the act of further looking. The response which by repetition becomes habitual has a stimulus which is gravitating steadily in the direction of a purely physical cause. When the response is wholly unconscious, the stimulus is a stimulus only in the sense that it is a link in a causal sequence. It is no longer a stimulus in the sense which links up stimulus and response in a correlative whole, within which the constituents of the whole undergo progressive and concomitant modification.

It is perhaps unnecessary to say that this interpretation of the relation between stimulus and response is the doctrine set forth by Professor Dewey in his article on 'The Reflex Arc Concept in Psychology.'[1] The brief comments on the doctrine in the foregoing paragraphs are intended mainly to indicate the direction from which, in the view of the writer, the correct interpretation of psychology is to come. If we place ourselves at this standpoint, we are in a position to accept the contention that psychology is a science which has to do with behavior. We get rid of the obscurities and ambiguities which are inherent in the current conceptions of sensations and images and of mental states generally; and at the same time we guard against the danger of taking behavior in a sense which permits the distinctive and significant trait of conscious behavior to disappear from view. In giving proper recognition to the peculiar character of the stimulus, we are led to interpret the current doctrines of such processes as attention, association, imagination and memory as simply formulations of the changes which stimuli undergo in their function of controlling response. In

[1] This REVIEW, 1896.

shifting these processes from 'consciousness' to things, we lay down an intolerable burden of mystery and contradiction. These processes no longer appear as independent facts, to be analyzed back somehow into pre-existent elements, but are interpreted solely with reference to the behavior with which they are correlated and to which they ordinarily furnish the most direct clue. While it is entirely legitimate and frequently necessary to emphasize response rather than stimulus, the proper goal of all psychology is to give a description of behavior in so far as it is determined by this unique form of control.

Psychology as thus understood is not open to the criticisms that are urged against current introspectionism. It does not call for an attempt to 'reconstitute' the experience of the subject, but rather to ascertain whether and in what specific manner stimuli exercise this peculiar type of control. As to introspection, its distinctive trait is neither its character as a method, nor the nature of its subject matter, but its problem or aim. I have contended in this paper that in conscious behavior the stimulus undergoes what Professor Dewey calls a process of reconstitution, the goal of which is a stimulus capable of evoking a final response in which the confusion of the several partial responses is disentangled and harmonized. The peculiarity of introspection seems to consist in taking a stimulus which has been thus reconstituted so as to be effective for such response and treating it as a candidate for a different process of reconstitution, the purpose of this latter process being to discover the physical and physiological conditions which are involved. This happens, for example, when we interrupt the drinking of tea in order to analyze the taste. As a result of this manœuvre, the taste undergoes a peculiar change,[1] so that a scent and a temperature appear. This fact is not to be taken as evidence that these sensations existed as primordial psychic constituents of the original stimulus, but that the sense-organs thus indicated help to determine the response in the drinking of tea. And similarly the discovery of overtones, of brightness and intensity, and other facts per-

[1] For a more extended discussion of this type of change I may refer to a former article, 'The Method of Introspection,' *Jour. of Phil., Psych. and Sc. Methods*, Feb. 13, 1913.

taining to the stimulus that is analyzed are not to be read back as sensations or their attributes, but should be taken as clues to the presence of certain physical factors. This same analysis of the stimulus is physics, if we drop the reference to the behavior of an adaptive organism.

In conclusion it may be pointed out that the procedure involved in this standpoint is necessarily of an objective and experimental character. The behavior which is studied is the behavior of an organism; the factors which are sought are physical and physiological in character. It is only by further progress in the direction of objective methods and objective description that psychology can free itself of the reproach which is heaped upon it by members of its own household and take the place which rightfully belongs to it in the community of the sciences.

THE SELF AND THE EGO[1]

BY KNIGHT DUNLAP

I have been asked to make clearer my distinction between the 'Self' and the 'Ego,' and I think I can do so by explaining more fully my fundamental psychological postulates.

My conception of the empirical self and my further notion of the Ego as an essential presupposition of psychology, which I have set forth in Chapters XVI. and XX. of "A System of Psychology" are organic parts of a general view which discards completely the psychophysical theory of Descartes, which theory, or rather Malebranche's amplification thereof, has become so firmly embedded in modern thought that it is usually accepted without question by psychology, science and common sense. It is necessary that I make this general statement, for I admit without hesitation that the attempt to put my notions concerning the Ego into orthodox Cartesian terms is useless.

In the first place, I postulate the reality of the sensible world. Those who choose to postulate its unreality are free to do so, just as those who postulate the proposition that two straight lines may be drawn through a given point parallel to a given line are free to do so. As there is built up on this postulate of parallel lines an entirely coherent system of geometry, so on the postulate that the world of sensible things is unreal and that reality lies in the world of non-sensible matter, an entirely coherent system of psychology might be constructed. I do not think it has yet been done, however.

In the second place, I insist on a strict use of the term *consciousness* (or its equivalents), and the discrimination of the two ways in which that term is currently used. If we use it for the awareness, or being aware of, a datum we have no

[1] A contribution to a symposium before the Southern Society for Philosophy and Psychology, at The Johns Hopkins University, April 9, 1913.

business to use it also for the datum itself (say a sensible datum or color, which for exactness we may call a *sentiendum*[1]). If we do use it promiscuously in both ways, we introduce into psychology the same lack of significance which would be introduced into a lease by using 'lessee' and 'lessor' interchangeably in the same document.

It is curious that psychologists in general seem to have been bent on the ignoring of the distinction between consciousness and its object. James recognized the distinction very clearly, but buried the evidence so deep in his *magnum opus* that most of us who were brought up on James never had a gleam of it, but were taught that the 'state of consciousness' was both the awareness of the red, and the red color itself, whereas it was, for James, neither.

The distinction between the content and the consciousness of it: between the *sentiendum* and the *sentiens*, may perhaps be made clear by supposing for the moment that a color exists whether anyone is aware of it or not. There is, let us say, a spot of red on the wall. I may see that red spot and attend to the *red*. It is the *red itself* of which I am speaking now, not the ether vibration. I never perceive directly the ether vibration. I may never have heard that there are such things. But I see the *red*. Now, let me close my eyes; let everyone else in the room close their eyes; let God close His eyes. Let us now all think of things other than red: of the high cost of living; or one of Glück's operas, for example. Assume that the red is unchanged, as for all we know, it may indeed be. Now the red, still existing but unperceived, is the sentiendum, the potential content. When I again open my eyes and turn my head in the proper direction, the red goes right on existing, but awareness of the red is now added. The red is unchanged, but becomes a 'content of my consciousness.' There are then two factors to be reckoned with: the red and my awareness of it.

The distinction holds, while red is a content, no matter whether the red exists 'out of consciousness' or not. The supposition that red exists when not perceived brings out

[1] See 'The Nature of Perceived Relation,' Psychological Review, 1912, XVI., p. 416.

sharply the distinction between the red and the perception of it, but the truth or falsity of the supposition has no effect on the situation while red is being perceived. It is as if we had before us a quiet, smooth-shaven man. By assuming that when he grows a beard, he will still be quiet, I may bring out the distinction between quietness and beardlessness, although the actual fact may be that when he grows whiskers, he becomes noisy and violent.

Once recognized, the distinction between the content and the consciousness simplifies the business of the psychologist immediately and immensely. For one thing, the elaborate doctrine of copy-images, with their many contradictory features, may be thrown overboard at once.[1] And I may remark in an aside that I believe that if this dead weight be jettisoned, psychology may escape the utter shipwreck in which, in the estimation of all non-psychologizing scientists and the estimation of many psychologists also, it seems now to be tossing.

We have, therefore, two problems before us. First, the analysis of the objects of content, in the attempt to discover the number of elements and their peculiarities; the study of the processes in content; and the study of the conditions (physical, in the broad sense) under which the various forms and elements appear. Parenthetically I may remark that the lions which have long stood in the way here are seen to have neither teeth nor claws, when we recognize that we are not dealing directly with consciousness. The often repeated statement that however we may analyze the concrete experience into elements, the experience was just as unitary as the elements, is seen to mean merely that whereas the content of an experience may be complex, the consciousness or experience of that content is not complex; or rather, that we do not analyze it.

In the second place we have the study of the conditions of consciousness. We can never directly observe consciousness, since consciousness is always the observation of a content.

[1] On the question of 'imagery' I shall, at the suggestion of one of the most discriminating of the reviewers of my book, attempt to make myself clear in a separate article.

We can, and must, however, examine the physiological conditions of consciousness; and also its logical conditions.

Consciousness, we find, has degrees. I am attentive or less attentive; the content is high or low in vividness. These things are partially synonymous. 'Consciousness,' 'attention' and 'vividness' are ways of expressing the fact of awareness in various degrees. To this I will return in a moment.

We may also be conscious in different ways, irrespective of differences in vividness. We may be conscious of content which is said to be 'present'; this consciousness I prefer to call by the old name *intuition*. Or we may be conscious of content not present. This we call *imagination*. Please take notice that these statements are not definitions. 'Present' content means merely 'intuited content' and 'not present' content means merely 'not intuited' content or 'imagined' content.

Other ways of being conscious are distinguished, the classification having possibly (but not certainly) no reference to the two classifications just mentioned. Consciousness of certain sorts of content is called *sensory* apprehension or *sensory* intuition: sometimes it is called *sensation:* but the term 'sensation' is more frequently used for the 'sentiendum.' The confusion is here so great that we would do well to exclude the term sensation from scientific discourse. Consciousness of certain other sorts of content is called *intellect*, popularly at least, and should be so called scientifically. Whether we are to apply the term 'feeling' to a way of being conscious, or to the affective content, is yet to be decided. At present it is applied equally to both.

Another division is between *introspection* and whatever is left over from that; non-introspective observation. Although I have no use for the orthodox *theory* of introspection, I propose to use the term, and to define it and use it as it is actually used by modern psychologists; namely, to signify the observation of kinesthetic and somatic sensations, and of affective contents. The only difference between my usage of the term and the usage of psychologists in general is that I explicitly define it as I use it, and that others sometimes, but not uni-

formly, include under it the analysis of the ideal content of imagination (so-called 'images').

Now I have outlined a simple system of psychology which covers all the ground in a way which seems to me to offer greater possibilities in the way of research than does the Cartesian method. I have provided for the recognition of a Self, which is basally the body, as constituted by the kinesthetic and somatic sensations, and by the 'feelings.' *Introspection* is rightly called *self-observation*. Emotions, since they are fundamentally complexes of bodily sensations and feelings, are rightly said by philosophers of all ages to be modifications or passions, of the Self. This, of course, does not exhaust the Self, but merely specifies the essential basis. The relations of this constantly experienced, and in itself relatively constant basis, to the more variable objects of 'external experience,' are also important. I am not at present interested in delimiting the Self; I do not know that it is possible to show where the Self leaves off and the non-Self begins. I am merely interested in identifying the concrete basis, or I might say, the core of the Self. I might go on another tack and show the importance of this Self, or rather of the rhythmic changes which are inherent in it, in determining our thought processes, but that is also out of the present line of interest.

I have now covered by title all the ground which can be studied, but I have also definitely involved something (if I may beg the use of the word *thing* in a rather vague sense), which cannot be studied, although the involution of the 'thing' cannot be escaped.

There is more to be considered than the content and the consciousness. Each awareness has a reference other than to the content. This may be made clear in the following way. Suppose three items of content, which we may designate as *a*, *b*, and *c*. Suppose I am aware of *a*, then of *b*, and then of *c*. The situation is quite different from that in which I am aware of *a*, another person aware of *b*, and a third person aware of *c*. This is so, even if we neglect the differences in *a*, *b* and *c* as they appear to three different persons. Neither is it true

that the awareness of *a*, *b* and *c* by myself establishes, or is the establishment of, a specific relation among the three. Assuming the existence of the three, there is no relation among them after I have perceived them that did not exist before I perceived them. This is demonstrable through the fact that my perception of the three items is not dependent on the perception of any particular relation among them. *a* may be brighter than *b*, and *b* than *c*, but I may successively perceive the three and not notice these relations at all. This proposition is true for any other relation that can be named. It is clearly not plausible that some relation, not specified, can account for the fact that I perceive all three.

The fact that I perceive all three, twist it as we may, remains to the end an ultimate, utterly inexplicable fact. The important thing about the situation that we are supposing is that they, the three items,—*a*, *b* and *c*,—are perceived by the same I. The perceptions are not the same; they may be temporarily separated by considerable intervals. What is the identity? Merely the identity of the I, the Ego. Manifestly, that which is identical in the case of the three perceptions is not that which is not identical. There is, therefore, the Ego, which must be discriminated from the awareness, perceptions or experiences which refer in common to the Ego.

Experience, consciousness, or whatever we may call that 'knowing' which James calls 'the most mysterious thing in the world,'[1] is quite clearly a polarized affair. At one end it refers logically to that which is known; at the other, to that which knows. Yet it is not in strict accuracy to be called a relation between the two, for a relation, as it is elsewhere understood, is between two objects, or knowable items, thus involving the possibility of this very consciousness of which we are speaking. We must be satisfied with considering consciousness as absolutely unique and irresolvable; there is nothing else in its class. We may call it an act, a relation, a quality, a process, a machine, or anything else, if we understand that these terms do not really describe it; for all of these terms refer primarily to objectivity and are intelligible only because we refer them NO

[1] James, 'Principles of Psychology,' I., p. 216.

to definite objective facts, and hence in applying them to consciousness we treat it as if it were objective.

There are thus three sorts of things which are immediately postulated as soon as we begin to talk of experience. *First*, the items which are experienced. *Second*, the experiencing of these items. You can no more talk about the objects without postulating the experience of these objects than you can talk about experience without postulating something which is experienced. *Third*, in postulating the object, or in postulating the experience of the object, you postulate that which experiences. We cannot talk of experiencing without an I which experiences; I can talk of the experience which I 'have,' but not of any which I don't 'have' or which is not exactly analogous to that which I 'have.' And it is certainly true that I 'have' no experience which I don't 'have.' We might suppose the existence of uncoördinated experiences, but these are then by hypothesis experiences which I do not have and which no other Ego 'has,' and hence are not to be considered as anything more than hypothetical. In other words, all experiences which are other than merely hypothetical, intrinsically postulate the I.

But what have we postulated when we have postulated the Ego? Nothing more, so far, than a certain coördination of experiences. There is one coördination of experiences which is expressed by saying that two experiences may be of the same object. It may be held that such a coördination does not exist: that no two experiences can be of the same object: this proposition is, however, itself an arbitrary postulate, a postulate, that is, which is in no wise necessary. We can avoid both this and the contrary postulation by saying that *if* two experiences are of the same object, there is *ipso facto* a correlation of experiences, and we may call that the *objective* correlation. Now all we have been claiming is expressed by saying that in addition to this possible objective correlation of experiences, there is an undoubted and unavoidable correlation of another kind, which we may conveniently call the *subjective* correlation, thereby fixing explicitly our use of the term subjective. This correlation is one of identity, and just as we

name the identity in the case of the objective correlation the *object*, we name the identity in the case of the subjective correlation the *subject*, or *Ego*. The object can be observed, and hence we can say more about it than merely to name it. The subject, however, is not observable, but is that which observes, and hence we can do nothing but name it, which we do by calling it the subject, the Ego, or that which observes.

It may be objected that the subject really is more than the observer; that it feels and wills as well as knows. This objection cannot be maintained. What we loosely call 'to feel' is really to observe or experience an affective content or feeling. To will is to experience a particular thing, namely, a feeling of desire or repugnance, in connection with some other content. The sole function of the Ego reduces absolutely to that of knowing, or, in other words, the sole subjective correlation is the correlation of experience.

DISCUSSION

THE PHENOMENA OF INDIRECT COLOR VISION

BY J. W. BAIRD
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I. My attention has been called to a recent thesis from the Bryn Mawr laboratory[1] in which the work of previous investigators of the color sensitivity of the peripheral retina is subjected to a vehement criticism.

Dr. Rand's attack deals chiefly with the question of the intensity of visual stimuli; and her arraignment of her predecessors is based almost exclusively upon her unique definition of intensity,—a definition which I believe no other investigator of the psychology of vision, with the exception of Dr. Ferree, would be willing to accept or even to consider seriously. Dr. Rand states that "intensity of stimulus will be used (throughout her paper) to indicate the *energy of light-waves* coming to the eye. Intensity of sensation, or apparent intensity, will be used as its correlative subjective term. So used, it will signify merely *energy or voluminousness of sensation and will have no reference whatever to the white-value of a color* (italics mine) . . . and the terms brightness and white-value will be used interchangeably to indicate the lightness or darkness of a color" (p. 20).

Starting out from these remarkable definitions she, naturally enough, reaches an equally remarkable result,—namely, that it is possible to 'standardize' visual stimuli by simply measuring their temperatures.[2] Armed with these two weapons,

[1] Gertrude Rand, 'The Factors that Influence the Sensitivity of the Retina to Color: A Quantitative Study and Methods of Standardizing,' *Psychological Monographs*, 1913, 15 (Whole No. 62), 166 pp.

[2] This method of determining the temperature (or the physical energy) of a beam of light did not, of course, originate in the Bryn Mawr laboratory. The method was described some twenty-five years ago by Langley, who pointed out however, even at that early date, that the range of its applicability is exceedingly limited; for in 1888 Langley warned his fellow physicists against the error of supposing 'that the luminosity

—her definitions, and her method of 'standardization,'—she engages in a merry tilt with her predecessors; and she endeavors to show that previous investigations of peripheral color vision have been, for the most part, a pathetic succession of misdirected efforts and ludicrous blunders.

Dr. Rand informs us, for instance, that Hegg's 'method of equating (the intensities of color stimuli) cannot warrant any conclusion concerning the relative limits of peripheral sensitivity to the different colors' (p. 32); and that Bull's 'method is an anomaly, and so far as the present writer knows is not justified in any investigation of color sensitivity that has yet been proposed' (p. 26). She alleges that Hess's 'object was primarily to furnish Hering with experimental evidence that would enable him to refute the Young-Helmholtz theory' (p. 26); Hess's 'assumption begins with a fallacy . . . (and) the assumption is itself incorrect' (p. 29). His test 'was both incomplete and wrongly devised' (p. 52); he 'used' his method 'very inadequately' (p. 40); he 'did not use the proper conditions of brightness of screen' (p. 50); and 'his test was thus again rendered unfair by his lack of proper conditions' (p. 50). As for Fernald's investigation, 'one may express surprise that work so sketchy should be considered as warranting any conclusion whatever' (p. 33). And Thompson and Gordon are found to have sinned with Fernald in employing a 'crude method' which 'lacks the first essential of standardization' (p. 60). And the present writer has apparently been guilty of every sin in the calender of science. My 'conclusion is apparently based upon a loose construction put upon the meaning of certain terms' (p. 13). "Like his predecessors, Baird also concludes beyond what is justified by his method of working" (p. 32). My blunders are due to 'failure to read carefully' (p. 53); and I have been guilty of misinterpretation in certain instances, and of mis-

of a color is proportionate to the energy which produces it.' He estimated 'that the same amount of energy may produce at least 100,000 times the *visual effect* in one color of the spectrum that it does in another.' (Italics mine.) (S. P. Langley, Energy and Vision, *Amer. Jour. Science*, 1888, 3d series, **36**, 359–379; see also *Phil. Mag.*, 1889, 5th series, **27**, 1–23.) From the foregoing one may see how inadmissible it is to employ such a method for the 'standardization' of visual stimuli.

representation in certain other instances. Ferree and Kirschmann, however, are among the favored few who escape Dr. Rand's general condemnation.

The criticism which is here urged against my work cannot be evaluated without a résumé of the situation. In a paper published in 1905[1] I mentioned the familiar fact that a 'color zone' upon the retina is found to be more widely extended in a peripheral direction when the stimulus employed in the exploration is more intensive;[2] and I also mentioned the consequence which obviously follows from this obvious fact,—namely, that no determination of the relative widths of color zones can furnish results which are really comparable with one another unless the stimuli employed have first been equated in brightness. A survey of the literature showed that Aubert had been the first investigator to realize the necessity of equating the intensities of his stimuli; that the same principle had subsequently been advocated by Chodin, Bull, and Hess; and that Landolt, Abney, and others had also shown that width of 'color zone' varied with intensity of stimulus.

Dr. Rand ascribes the discovery of this principle to Bull (1881); and she endeavors to show that I have erred in my statement of the views of Aubert (1865), Landolt (1872), Chodin (1877), Abney (1898) and others.

(*a*) In the case of Aubert, Dr. Rand directs her criticism against the fifth of the six paragraphs in which I summarized the findings and the conclusions of this investigator. She apparently sets out with the assumption that a one-to-one correspondence should somehow obtain between the several paragraphs in Aubert's statement of his summary, and the several paragraphs in my statement; nor is she daunted by the fact that the number of paragraphs is not identical in the two instances. She proceeds to compare one of my paragraphs, selected by herself, with one of Aubert's paragraphs, also selected by herself; and, naturally enough, she finds that the

[1] J. W. Baird, 'The Color Sensitivity of the Peripheral Retina,' Washington, D. C. Published by the Carnegie Institution of Washington, 1905, 80 pp.

[2] Various other factors which affect the width of the color zone were also mentioned and discussed; but these need not be considered here.

two paragraphs are not identical in meaning. She next alleges that when Aubert employs the term *Helligkeit* without a qualifying phrase he 'commonly refers to the intensity or brightness of the general illumination' (p. 16), and not to the intensity or brightness of the stimulus itself. From all of this it is to be inferred that I have mistranslated a paragraph in Aubert's summary, and that I have misinterpreted the meaning of *Helligkeit*. Dr. Rand concludes this remarkable argument with the statement that she 'is compelled to say that in a careful reading of all the articles by Aubert contained in the long list to which Baird refers, she is unable to find a single statement that would justify the conclusion that Baird has drawn' (pp. 16 f.).

In her further reading of the literature, however, she makes the disquieting discovery that both Bull and Hegg agree with my interpretation of Aubert; but instead of now withdrawing her criticism of my statement she counters with the additional charge that I have at any rate failed to give the page reference to the passage in question (p. 44); and she later repeats that 'Aubert does not in the references given by Baird' 'claim that brightness difference affects the sensitivity of the retina to color' (p. 52).

In reply to Dr. Rand's criticism it need only be mentioned that Aubert's 'Physiologie der Netzhaut' contains the following passages: "Denn dieselben Betrachtungen, die wir in §55 bei Gelegenheit des directen Sehens über die Helligkeitsverhältnisse der Pigmente und deren Einfluss auf die Wahrnehmbarkeit der Farben angestellt haben, finden auch hier Anwendung, so dass nicht zu eruiren ist, wie weit die Farben an und für sich Differenzen setzen. Wir werden daher nur sagen können: Contrast und Helligkeit der Farben sind von grossem Einfluss auf die qualitative Farbenempfindung, so wie auf die Grösse der Netzhautparthie, innerhalb welcher die Farben empfunden werden können" (p. 122). Section 55 to which Aubert here refers back contains the following statement: "Die Bestimmungen der Tabelle XV. sind also gewissermassen nur Brutto-Bestimmungen der Empfindlichkeit für Farben; zu einer Netto-Bestimmung müsste der Einfluss der

Helligkeiten eliminirt werden können, also *Pigmente von gleicher Farbenintensität* und Nüance auf einer Umgebung von derselben Helligkeit wie die Pigmente beobachtet werden. (Italics mine.) Solche Pigmente giebt es aber nicht, und da auch der photometrische Werth der prismatischen Farbentöne unbekannt ist, so erschient eine exacte Bestimmung des Gesichtswinkels, unter welchem die Farben empfunden werden können, überhaupt unausführbar" (p. 112).[1]

(*b*) Dr. Rand's criticism of my statement regarding Chodin is equally erroneous. She asserts that "Baird writes: 'Chodin remarks in his introduction: "It is self-evident that in comparing the retinal sensitivity to different colors, the color-stimuli employed must be of equal brightness and of equal saturation." But this very essential condition was not fulfilled in his own experiments' (see Baird, p. 20). Baird has here again made a misinterpretation. The rather free translation of Chodin's statement: 'Es bleibt nur übrig die Farben bei gleicher Sättigung und bei mittlerer Lichtintensität zu vergleichen' and the failure to read carefully the discussion following it are responsible, we presume, for the misinterpretation" (*loc. cit.*, p. 53).

A comparison of Chodin's statement with my statement (both of which are here appended in parallel columns) will show that Dr. Rand's charges of 'rather free translation' and of 'failure to read carefully' are wholly groundless.[2]

[1] The fifth paragraph of my summary reads as follows: "The relative extension of the different color zones cannot be determined with any degree of accuracy. Since the width of the color zone is a function of the luminosity of the stimulus, the color-stimuli employed in the determination of comparative retinal limits must all be equated in brightness. In the opinion of Aubert, the comparison of the relative brightness of stimuli of different colors is attended by such difficulties as to render its accurate accomplishment impossible, etc." The reader will note that that part of Aubert's statement which here deals with the effect of background is summarized in another paragraph of my paper (§1, p. 12). With regard to my alleged failure to include the foregoing passages in my published references to Aubert, it may be added that my paper contains a detailed reference to the page in Aubert upon which the first of the above citations is to be found; and this first citation itself specifically refers back to §55 wherein is to be found the second citation.

[2] Here as in the case of Aubert, my critic bases her charge upon her failure to find an identity of meaning in non-corresponding sentences which she has selected, apparently at random, from papers by these authors and from my monograph. In the present instance she has not even confined her selection to the page in Chodin which

Chodin's statement is as follows:

"Es ist selbstverständlich dass bei Vergleichung der Empfindlichkeit für verschiedene Farben diese letzteren von gleicher Sättigung und gleicher Intensität sein müssen." (A. Chodin, Ueber die Empfindlichkeit für Farben in der Peripherie der Netzhaut. *Archiv für Ophthalmologie*, 1877, 23 (3), p. 178.)

My statement is as follows:

"Chodin remarks in his introduction: 'It is self-evident that in comparing the retinal sensitivity to different colors, the color-stimuli employed must be of equal brightness and of equal saturation.'" (J. W. Baird, *The Color Sensitivity of the Peripheral Retina*, Washington, D. C., 1905, p. 20.)

In view of this statement by Chodin, in 1877, and of a similar statement by Aubert, in 1865, it is difficult to understand what justification Dr. Rand is able to find for her assertion (p. 25) that 'the first to recognize the need for making any sort of intensity equation of the stimuli used to investigate the relative sensitivity of the retina to the different colors was Ole Bull' (1881).

(*c*) Dr. Rand is in error again in her criticism of my discussion of the work of Landolt and Abney. She states that "Landolt and Abney . . . clearly use the term intensity to mean the energy of the light-waves coming to the eye" (p. 13). And again: "Abney was not at all concerned with the effect of the lightness or darkness aspect of the stimulus. . . . His purpose was to vary the energy of the light-waves coming to the eye by obstructing them by known amounts, and to ascertain the effect of this change upon the color limits. . . . Hence Baird is not justified in stating that Abney makes the brightness of the stimulus a factor in determining the color limit, etc." (p. 17). Dr. Rand's statement is in direct contradiction with Abney's own statement. Abney reports that he measured his stimuli not by a method which determined their physical energy in objective terms, but by a method which determined their brightness in purely retinal terms. (*Phil. Trans.*, Series A., 1898, 190, pp. 158 f.) His measurements of his stimuli were based exclusively upon subjective estimates of brightness; and he found that decrease in brightness of stimulus (through nine gradations) is invariably attended by a decrease in the extension of the

I cited as my authority. In my monograph (p. 20, footnote) I specified that the statement to which I referred is to be found on page 178 of Chodin's paper. Dr. Rand, however, selected her sentence from page 179 of this paper; yet she adds the erroneous statement that she selected it from the page which I cited (see Dr. Rand's paper, p. 53, footnote).

retinal zone (*loc. cit.*, p. 184). Dr. Rand states, however, in yet a third passage that "Abney and Landolt do not even claim that brightness difference affects the sensitiveness of the retina to color" (p. 52).

If any reader is interested in pursuing this matter farther, he will find that Dr. Rand has erred again in her references to the work of Landolt, Raehlmann, Klug and Bull; but it seems bootless to discuss these errors, or even to enumerate them in detail.

II. Several years ago Dr. G. M. Fernald reported that colored after-images may be obtained from stimuli whose colors are subliminal.[1] At the suggestion of Professor Titchener I repeated the experiments but my results were wholly negative.[2]

In a recent reference to these experiments, Dr. Ferree and Dr. Rand offered the following criticism: "Professor Baird reports negative results in every instance. With regard to this work the writers cannot help but observe that *Baird has failed to conform*[3] to the conditions which Fernald had said are essential for getting the phenomenon. Without drawing upon their own experiments for a knowledge of essential conditions, they will point out three conditions which *Baird has apparently failed to fulfil*,[3] the neglect of any one of which is amply sufficient to account for his results being negative. (1) Fernald lays great stress upon the use of a campimeter screen by means of the induction from which the brightness conditions were obtained which obscured the color in her stimulus. Baird used a simplified form of perimeter, how simplified Titchener and Pyle do not state. (2) Baird uses for the

[1] G. M. Fernald, 'The Effect of Brightness of Background on the Extent of the Color Fields and on the Color Tone in Peripheral Vision,' Psychol. Rev., 1905, 12, 405; 'A Study of After-Images on the Peripheral Retina,' Psychol. Rev., 1907, 14, 129 f.

[2] Instead of publishing my results I turned them over to Professor Titchener who was then interested in the more general aspects of this problem. (See E. B. Titchener and W. H., Pyle, 'On the After-Images of Subliminally Colored Stimuli,' *Proc. Amer. Philos. Soc.*, 1908, 47, pp. 366–384.) Titchener and Pyle did not publish a detailed description of my experimental procedure, nor a complete statement of my experimental findings. They did, however, quote excerpts from my results, to which they appended the remark: "It does not seem necessary to publish the full set of results, though the data are at the disposal of any one who may wish to consult them" (p. 378).

[3] The italics of this anti-climax are mine. (See also the italicized phrase on the next page.)

duration of the stimulation intervals of 30 to 40 seconds. Fernald is careful to state that the interval of stimulation should not exceed three seconds. (3) In her description of conditions Fernald states that the color should be exposed behind the opening in a campimeter screen, and the card upon which the after-image is projected should be slipped in between the colored surface and the stimulus opening. Thereby the campimeter screen and thus the larger part of the field of vision remains unmoved and the least possible incentive is given for involuntary eye-movement. With Baird's apparatus, however, *we would judge*[1] that the ground upon which the after-image was projected must have been moved in between the stimulus and the observer's eye, thus exerting a strong incentive to drag the fixation with it."[2]

All three of these criticisms are based upon misapprehension of facts. If my critics had read my description of the apparatus and the procedure employed in these experiments[3] they would have learned (1) that the induction-screen in my apparatus was identical, in every essential particular, with that employed by Dr. Fernald; (2) that the duration of stimulation recommended by Dr. Fernald was employed in my experiment (when I failed to obtain positive results in exposures of two to three seconds' duration, I varied the duration of stimulation, in a few additional experiments, down to one second and up to one minute); (3) that the card, upon which our after-image was projected, invariably appeared behind the screen as Dr. Fernald recommended.

Since the Titchener-Pyle paper was published, Dr. Fernald has demonstrated to me that my failure to obtain positive results in these experiments was due to the absence of a sufficiently intensive light-adaptation during the preëxposure period; Dr. Fernald's demonstration has convinced me that the paradoxical after-image is a genuine phenomenon, and

[1] The italics of this anti-climax are mine. (See also the italicized phrases on the preceding page.)

[2] C. E. Ferree and G. Rand, 'Colored After-Image and Contrast Sensations from Stimuli in which No Color is Sensed,' Psychol. Rev., 1912, **19**, pp. 198 f.

[3] I am informed by Professor Titchener that Dr. Ferree and Dr. Rand did not avail themselves of his offer to furnish these data.

that it is not a product of chromatic adaptation as I formerly believed. I gladly take advantage of this belated opportunity to call the attention of the reader to the profound significance of Dr. Fernald's findings; in my own opinion, they rank with the most important contributions made in recent years to the literature of color vision.

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THE PSYCHOLOGICAL REVIEW

THE MENTAL AND THE PHYSICAL[1]

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THE DOUBLE-ASPECT VIEW

The mind-body relation is the Wandering Jew of science. It is ever re-appearing when other issues are dead and buried. For centuries the same opposing interpretations of the relation between the mental and the physical have fought for recognition. We still find advocates of the two monistic theories, materialism and animism; more numerous are the champions of dualism in its two forms, interactionism and parallelism; some welcome the double-aspect view; while many in despair adopt the agnostic attitude.

The psychologist of today may be content to leave the broader aspects of the problem in the hands of the philosopher. He can scarcely avoid taking some attitude on the relation of consciousness to nervous activity. It is true the behaviorist urges us to limit investigation to the objective side. Yet few among us are ready to abandon the empirical study of consciousness. Fewer still would hark back to Thomas Brown and base psychology on introspection alone. The tendency today is to study mental phenomena in relation to the activity of the nervous system. Even the laboratory investigator, with his fine scorn for verbal jugglery, finds himself impelled to adopt some hypothesis, or at least to frame some concept of that relation.

[1] Address of the president, before the American Psychological Association, New Haven meeting, December, 1913.

At present interactionism and parallelism seem to preponderate. The majority of psychologists adopt one or the other. The claims of each standpoint have been ably upheld and combatted by many recent writers. Carl Stumpf[1] in Germany, C. A. Strong[2] in America, and William McDougall[3] in England are familiar instances.

Were it permissible to adopt the test of inconceivability we should find ample grounds for rejecting both interactionism and parallelism. To some of us the interaction of brain and consciousness would seem to imply that two incommensurables may possess a common factor. It is as though one should attempt to guide a motor car by talking to the steering wheel. On the other hand parallelism, as usually expounded, suggests that the chauffeur's hand is beside the wheel but does not grasp it: the chauffeur indicates the direction with a wave of his hand; the wheel, turning of its own accord, responds exactly.

Now we may grant that a theory of the universe is not to be demolished by a casual jest. And still we may not wish to undertake a logical refutation of any metaphysical theory. The very bases of logic are undergoing reconstruction, and the times are not yet ripe for a final world-view. The scientific psychologist, however, does not aspire to a vision of the naked ultimate; he seeks only to clothe the observed facts in a garment which will make them duly presentable.

I shall try to show, first, that the double-aspect view is the most satisfactory formulation of the mind-body relation; second, that this hypothesis enables us to extend the mechanistic interpretation, so fruitful in other fields, to the objective data of psychology; and finally, that it leads naturally to a two-aspect psychology—the psychology of introspection and the psychology of behavior.

Historically, the double-aspect notion has suffered in two ways: In the first place, it lacks a convenient name. And further, to understand what such a relation means we need a suitable analogy taken from every-day experience; whereas a satisfactory illustration of this sort has never been given. In

[1] Presidential Address, 4th International Congress for Psychology, 1896.

[2] 'Why the Mind has a Body,' 1903.

[3] 'Body and Mind,' 1911.

consequence of this double handicap the two-aspect view has been misunderstood and misrepresented. It has usually been identified with parallelism or with some form of monism. As Professor McDougall points out, it is neither. He calls it a form of 'identity-hypothesis.' It is rather a dualistic monism, —a *monodualism*, to name it in conformity with other theories.

To make this view clear we need an observable instance of some 'thing' exhibiting two aspects, inseparably bound together, yet manifest in two distinct ways. The classic illustration of concavity and convexity is inadequate, for concave and convex surfaces are both perceived in the same way.

The *surface-mass* relation of matter offers a better analogy, and helps to a clearer understanding of the mind-body relation. As data of experience the *surface* of a body and its *mass* or weight are perceived by two distinct senses. Divide a lump of earth or metal into as many parts as you will, your eye perceives only the outer surface of the parts—you never see the mass within. Heft it, and you feel only the weight—the surface is never a datum of muscular perception. Alter the shape as you will, the relations of the constituent particles are altered *pari passu* with the change of surface relations. Mass and surface change conjointly; they are inseparably bound together; they are two radically distinct aspects of the same thing.

This illustration enables us to conceive the possibility at least of the double-aspect theory. I grant that it *proves* nothing. It is nothing more than analogy. Yet it serves to picture the relation between nervous system and consciousness without supposing them to be two distinct things, either interacting on one another or separate and parallel. The analogy is doubtless imperfect. Mass does not correspond, character by character, to the mental aspect or inwardness of the world; surface does not correspond to the physical aspect or outwardness of the world. The figure simply makes the monodualistic relation comprehensible.

This is of no small importance, inasmuch as many writers have affirmed that the double-aspect view is absurd or meaningless. McDougall says: "A thing or being or process can appear

under two different aspects, can manifest itself in two different modes, only if and when both aspects are apprehended by the mind of some observer; either one observer must occupy the two standpoints successively, or two or more observers must apprehend it from the different standpoints. Now, in the case of the physical and the psychical processes which are said to be two aspects of one process, there is no such observer occupying the inner standpoint and apprehending the inner or psychical aspect of the real event, except in the altogether exceptional case of the introspecting psychologist. . . . These considerations seem to me to raise an insuperable objection to this form of the identity-hypothesis; namely, there is lacking, except in certain special cases, any observer occupying the inner standpoint."[1] Notice, first, the anthropomorphic 'observer' whom Professor McDougall insists on introducing; and second, the persistence of the old Kantian assumption that there *must* be a basal substance, a Ding-an-sich underlying the two aspects. Metaphysicians have demonstrated more than once the logical impossibility of reconciling dualism with monism. Yet one may venture to challenge the validity of any disproof which rests on apriori foundations. The knife of logic is not yet ground to sufficient keenness for cutting metaphysical knots. We cannot forget that the spherical form of the earth was disproved by the absurdity of imagining men head downward at the antipodes. Despite apriori compulsion the scientist may content himself with the inwardness and outwardness which he actually discovers among the data of experience; the problem of a tertium quid or Ding-an-sich may be left to some future generation of scientific philosophers. To theorize at present as to what the 'underlying reality' *must* be and how it *must* appear, would be like Columbus's attempt to draw a map of America before his first voyage.

Dualism offers us two alternatives: either the mind influences the physical world, or else it is an epiphenomenon and we are only conscious automata. But is not a third solution possible? Do the changes of mass in a body *produce* its changes of surface? Do surface changes *cause* the alterations in its mass relations?

[1] W. McDougall, 'Body and Mind,' pp. 157–59.

There is surely but one set of changes involved, though they may be regarded from two distinct standpoints. Just so it is conceivable that neural changes, viewed from another standpoint, are not the cause but the *external appearance* of mental changes.

In the surface-mass relation one aspect of the change is perceived by the eye, the other aspect by the muscle sense. Similarly, in the neuroconscious relation one aspect is objective —it is perceived from without; the other aspect is subjective —it is the conscious experience of the living organism itself. The parallelist errs in divorcing the two. The outwardness and inwardness of reality need not be regarded as two independent series, but as two manifestations of the same series. There is a boot for each leg. Mind is no more an epiphenomenon of brain activity, than the brain center is a hypophenomenon of consciousness. It is clear that we are not reduced to a choice between the causal relation and absolute separation. Changes of surface and changes of mass do not influence one another, neither are they independent. Just so the monodualist regards the activity of consciousness and the activity of the nervous system as neither causally related nor parallel. They constitute one single process, observable in two ways.[1]

This view is in harmony with the potential energy theory proposed by Professor Montague in a recent paper.[2] If he regards consciousness as the *inner aspect* of potential energy, then he merely adds a limiting clause to the double-aspect theory. The restriction of consciousness to *potential* energy may admit of empirical verification or disproof at some future time. If on the other hand Dr. Montague believes that potential energy is *another name* for consciousness—that the two are identical—his assumption seems like identifying visual surface with the mass which we lift.

[1] An 'aspect' or *way of regarding phenomena*, does not affect the succession of the phenomena themselves; so that the influence of one aspect on another has no significance for our problem. Neither is it essential to determine which aspect represents the world the more faithfully. The present writer has found the surface-mass analogy most helpful in clarifying his own world-view. It has stood the test for at least 15 years.

[2] W. P. Montague, 'Consciousness a Form of Energy,' Essays Philosophical and Psychological in Honor of William James, 1908. (Cf. W. Ostwald, 'Natural Philosophy,' p. 78; Holt, 1910.)

The double-aspect theory simplifies the genetic problem of consciousness. We need not speculate at what point in the animal series or in the human embryo consciousness arises. For we can assume that consciousness or its prototype pervades the whole organic world. If consciousness is not a term in the series of physical events, then the fact that amœba and other lower creatures act mechanically and uniformly is no ground for denying that they possess consciousness.

It has been assumed that human consciousness exists merely in connection with the higher centers. This means only that the 'consciousness wherewith we are conscious' (that is, our personal stream of thought) is limited to cortical activity. A broader view of mentality is obtained when we posit an experience side corresponding to the lower centers as well. True, this is no part of our own experience; but neither do all the activities of the *higher* centers result in experiences which join up with our personal life. The pathological states of split-off consciousness are by no means the only examples in point. There are instances in normal life as well. We remember the strokes of a clock after it has ceased striking, though the impressions failed to reach our consciousness at the time. We solve a problem in our sleep or while attending to something else. All we can actually affirm in such cases is that the experience in question does not form part of our personal stream of consciousness. To deny that there is *any* consciousness connected with this cerebral activity is on a par with denying consciousness to our best friends because their experiences are not part of our own consciousness.

To each one of us the inner aspect of the world includes only that which himself experiences. The same induction which leads us to attribute consciousness to other human beings points to its existence in lower animals and in the lower centers within our own body. If consciousness is the selfhood of the organism, it may well be present in lower species and in the lower centers of man.

We should avoid an anthropomorphic interpretation of this lower selfhood. The consciousness of amœba and the consciousness of a spinal center are surely very different from

the consciousness of an organized human individual with his complex cortex and web of association fibers. To endow the simple protozoan with a consciousness similar to ours is absurd. It is like the uncritical attitude which attributes human rationality to trained horses, in defiance of all experimental evidence concerning their learning processes. We can only assume in amœba and his fellows some inner mental life of a very simple sort, which with the evolution of organic life gradually evolves in complexity into the human type of consciousness.

If the term 'consciousness' seems to imply too much intelligence, let us call the simplest forms of inner mental life *proto-esthesia*, and reserve consciousness for the higher phenomena. The essential point is to avoid assuming a fictitious break in the chain of mental evolution. Referring again to our analogy, the observer finds no division of material objects into masses with surface and masses without surface. Similarly, biology and animal psychology have found no dividing line in the organic scale between animals with consciousness and those which lack it. As in many other phases of nature there seems to be no real discontinuity—only a gradual evolution.

Professor Holt's[1] theory that the simplest data of human experience may be resolved into more primitive mental atoms, offers a satisfactory corollary to this view. The increasing complexity of consciousness year by year in the human child and the evolution of nervous complexity in the animal series are matters of observation. They strongly suggest that the initial form of sensation ('proto-esthesia') is something simple and undifferentiated. Professor Holt carries the notion of mental evolution to a rigorous conclusion. His view lends itself better to the double-aspect interpretation than to interaction or parallelism. In a recent paper Professor Herrick concludes that cortical activity consists in the 'coördination and integration of highly elaborated subcortical organic circuits';[2] this lends support to Professor Holt's view from the physiological side.

[1] E. B. Holt, 'The Place of Illusory Experience in a Realistic World,' *The New Realism*, especially pp. 350–355.

[2] C. J. Herrick, 'Some Reflections on the Origin and Significance of the Cerebral Cortex,' *Journal of Animal Behavior*, 1913, *3*, 236.

The Mechanistic Interpretation

One objection to a non-causal view of the mind-body relation demands special consideration. It is charged that unless interaction occurs between mind and body the universe is reduced to pure mechanism. If conscious activity has no effect on the sequences of the physical world, if thought is not a cause of action, then (it is argued) the living, thinking organism is an automaton and ethical responsibility becomes a myth.

Let us face the issue squarely. Scientific research points more and more in the direction of a rigid, mechanistic interpretation of the physical universe. It is time for scientists to recognize that the data of psychology conform to a uniformitarian pattern also.

Hitherto mental phenomena have been too often regarded as something apart from the world-order—something out of harmony with its regularity. In Newton's day the movements of the heavenly bodies were considered something apart from the movements of terrestrial bodies. The burden of proof and the fear of anathema rested on those who sought to harmonize them. Now the complexion of the world has changed. Chemists and physicists have demonstrated the uniformity and regularity of events in their domain. The doctrine of conservation is only one striking example. Physiologists are extending the same notion of uniformity to the more complex realms of cytology and organic processes. Animal psychologists have begun to trace out uniformities in the activities of organisms. Sociologists have discovered many regularities in the interaction of living creatures. All these sciences have done their best work under the inspiration of the uniformity hypothesis. If the glory of modern science is its determinism, why should uniformity be considered degrading in the sphere of mental events alone? If the 'reign of law' means automatism, should not the term automatism be purged of its stigma? Is psychology to remain under the imputation of holding a primitive conception of the world from which the other sciences are fast escaping? Must we not amend the traditional interpretation of choice, reason, and volition, so as to bring these phenomena into harmony with the scientific conception of uniformity in nature?

Our latest psychological text-books indeed are free from the older anthropomorphic misconceptions. They analyze the rational and volitional processes into elementary data. Yet they generally fail to explain the standpoint which their analysis implies. Upon examining carefully ten typical American treatises on general psychology which have appeared within the past decade I cannot find in any of them a definite statement of the attitude of modern psychology toward the traditional view of choice, reasoning, and volition.[1] The writers do not make clear that we have ceased to regard the higher mental processes as arbitrary, indeterminate phenomena. It behooves those among us who accept a uniformitarian world-view, to proclaim their attitude in no uncertain terms.

In order to emphasize the automatic character of activity in the lower organisms, biologists are wont to declare that their tropisms do not manifest any signs of 'choice.' According to Jacques Loeb,[2] "We are here dealing with a forced reaction in which the animals have no more choice in the direction of their motion than have the iron filings in their arrangement in a magnetic field." With becoming scientific courtesy our biological friends accept the notion of choice prescribed by traditional psychology. With equal scientific candor they rule this notion out of their own sphere. Is it not time for psychology to indicate plainly that it no longer regards choice as something arbitrary or indeterminate? In the early days of Darwinism it was urged against Natural Selection that the process is really not selection at all. Yet when we stop to consider, is not natural selection a *most typical* example of choice? The choice of nature is not arbitrary: it is determined uniformly by natural processes, yet it results in the weeding out of some alternatives and the establishment of others. This, in the opinion of uniformitarians, is the highest type of selection.

[1] The works referred to are Angell, 'Psychology,' 1904 and 1907; Calkins, 'First Book in Psychology,' 1909; Dunlap, 'System of Psychology,' 1912; Judd, 'Psychology, General Introduction,' 1907; Meyer, 'Fundamental Laws of Human Behavior,' 1911; Pillsbury, 'Essentials of Psychology,' 1911; Read, 'Introduction to Psychology,' 1911; Thorndike, 'Elements of Psychology,' 1905 and 1907; Titchener, 'Text-book of Psychology,' 1909–10; Yerkes, 'Introduction to Psychology,' 1911. The authors selected are all members of this Association.

[2] J. Loeb, 'The Mechanistic Conception of Life,' p. 218.

The burden of proof today should rest on those who question uniformity in any sphere. Among biologists the debate is between mechanism and vitalism. Can vital phenomena be wholly explained in physical and chemical terms or not? The term *vital force* may be employed in two different ways: either to indicate our *ignorance* of certain physiological processes, or to designate a sort of process quite *distinct* from physical or chemical change. In the former sense it has no bearing on the dispute. In the latter sense it presents a definite problem for investigation. Now what is known about vital forces? The advocates of vitalism claim only that certain vital phenomena are *not fully explicable* in physicochemical terms. They have no alternative explanation to offer—only the name *Entelechy*, which serves to label the unsolved problems but does not advance one whit our understanding of them.[1] Driesch, for example, brings forward three separate proofs of vitalism;[2] every one of these rests on the *inconceivability* of imagining a machine so constructed as to perform certain processes observed in organisms. This is the inconceivability of antipodes over again. A century ago it was inconceivable that the chemical composition of distant stars should ever be known; and it is not many years since the earth's curvature rendered the notion of radiotelegraphy absurd. Mechanism is not synonymous with artificial machinery. Any process which involves only physicochemical changes is mechanistic, though it differ radically from a linotype or a refinery.

The organic processes are every year brought more and more within the domain of physics and chemistry.[3] Unless some new force is definitely discovered, should not the scientist assume that the unexplained processes harmonize with those already worked out?[4] Vitalism is a *possible* hypothesis, but it is not scientifically acceptable, for it stands without direct support. The present evidence from biology justifies the

[1] Cf. H. Driesch, 'Science and Philosophy of the Organism,' Vol. 1, p. 143: "But shall we not give a name to our vitalistic or autonomous factor *E*, concerned in morphogenesis? Indeed we will . . .. Let that factor . . . be called entelechy."

[2] *Op. cit.*, Vol. 1, pp. 146, 224; Vol. 2, p. 71.

[3] See J. Loeb, *op. cit.* especially p. 14 and p. 26; B. Moore, 'The Origin and Nature of Life,' *passim.*

[4] Loeb, *op. cit.*, p. 59.

assumption that physicochemical processes govern the growth and activity of organisms,—that uniformitarianism and mechanism are interchangeable terms.

Psychology should adopt the same attitude toward the phenomenon of choice. With the uniformities in behavior of simpler organisms staring up at us from below, and the social uniformities of human groups staring down at us from above, are we not bound to work under the assumption that human choice is fully determined by natural antecedents? The physical antecedents and consequents which constitute the outer aspect of choice are matters of objective study.[1] Only when these processes become *too complex* for practical observation, do biologists and animal psychologists admit the existence of choice in the traditional sense,—that is, as indeterminate. May we not suspect that they conjure a name to cloak their ignorance of certain antecedents?

The same tendency to keep alive the folk-lore of prescientific psychology appears today in the popular notions of *reason* and *rationality*. In the older psychology reason was supposed to transcend the natural world—to be a sort of intuition of truth vouchsafed to man alone. Even today reasoning is popularly regarded as yielding results superior to the events of nature or even running counter to them. Yet when we analyze the situation closely what does reasoning do but *follow the processes of nature?* Reasoning is correct only when it brings to consciousness the very results that processes of nature would themselves work out. As a mental process, reasoning is a special form of association. It differs from ordinary association in that it reaches conclusions which tally with the objective world. Any slip in adding a column of figures transforms the associative process from the rational or logical type to ordinary contiguity or similarity. It is vain for philosophers to assert that we cannot think of 2 and 2 as equivalent to 5. Any bookkeeper who has spent hours searching for an error of one cent in a trial balance knows that it is perfectly possible to think "$2 + 2 = 5$" time and time again. In other words, reasoning

[1] "Choice is a term based objectively on the fact that the organism accepts or reacts positively to some things, while it rejects or reacts negatively or not at all to others" (H. S. Jennings, 'Behavior of the Lower Organisms,' p. 330).

is valid, not when it transcends the uniform, mechanistic pattern of objective nature, but only insofar as it adheres exactly to that scheme.

This view of reasoning bears directly on the free-will problem. Suppose for a moment that voluntary choice *is* indeterminate. The highest type of volition, according to libertarians, is that based on reasoning. But the only valid type of reasoning, as we have just noted, is mechanistic. Hence, the more closely our voluntary choice follows the 'automatic' processes of nature, the higher it becomes. Hence, the effort of indeterminate volition as it perfects itself is to be rid of its own indeterminacy. Admitting, then, that indeterminate choice does exist, we find in the evolution of the organic world the following progress: (1) a stage of determinacy at the bottom, followed (2) by a supposed stage of indeterminacy or freedom of choice, this yielding (3) to the highest stage, which is determinacy once more. But why assume the second stage, which is contrary to all the trends of modern science? Is it anything more than a gratuitous assumption? Is not the hypothesis of indeterminacy a relic of the folk-lore stage of psychology? We are beginning to understand somewhat the physiological mechanism of control and inhibition.[1] The nervous integration involved in these processes depends on past and present stimulation. Are we not bound to assume that the mental aspect of voluntary activity tallies with the physical aspect?

The extension of the mechanistic interpretation to human choice and volition involves a re-interpretation of the ethical concept of responsibility. The moralist should relinquish his apriori notion of oughtness and gather his data from empirical sources; he should above all consult the alienist and child psychologist before attempting to construct the foundations of responsibility. The real science of ethics is concerned with the sense of responsibility which mankind *actually possesses*—not the hypothetical responsibility of a mythical man. The data of ethics are the actual motives of human action; science has no place for the paradox of forceless incitations.

[1] See C. S. Sherrington, 'The Integrative Action of the Nervous System.'

In all these cases of choice, reasoning, volition, responsibility, the difficulty is the same: the philosopher, the biologist, and the common man have not yet put aside the anthropomorphic psychology of earlier times. In our day it is no longer assumed that the movements of planets are guided by angels. We have ceased to regard gravity as a mystical substance. Our children soon abandon the idea that an automobile is propelled by a horse concealed within. Yet many persons still think of voluntary choice as determined by a man inside a man. They glorify chance by teaching a morality based on indeterminate activity. Let psychologists stand firmly against this anthropomorphic attitude and swing psychology into line with the other sciences. Let us emphasize the assumption that mental phenomena, including volition and choice, are uniform. Let us place the burden of proof where it belongs—on those who argue for an arbitrary element in choice, and on those who regard reason as a shrewd creature within us working counter to the universal processes of nature.

We need not include intelligence in this indictment, for the term seems already to have undergone a hopeful change of meaning. Animal psychologists find a measure of intelligence in the capacity of a creature to improve through experience; they no longer assume a man inside a dog to make him act intelligently. They study empirically the animal's progress in acquisition—his adaptation of behavior to changes in the environment, his growing facility in performance. The empirical science of animal intelligence appears to be gaining wide acceptance. It remains for human psychology to study in the same way the growth of rational intelligence in childhood.

The problem of teleology still towers forbiddingly athwart our path. In popular psychology foresight belongs exclusively to reasoning and intelligence. Intelligent acts and rational acts are said to depend on the consciousness of an *end in view*, which is to be accomplished by the activity; they are therefore characterized as *purposive* or *teleological*. Some years ago Professor Minot[1] summed up his theory of consciousness as follows: "The function of consciousness is to dislocate in time

[1] C. S. Minot, Presidential address before the A. A. A. S., *Science*, 1902, *16*, 1–12.

the reactions from sensations." His language seems to exclude sensation from consciousness. But despite this confusion his theory points out a possible biological interpretation of the relation of consciousness to purpose. Vision, smell—any distance receptor, in fact—may modify the reaction of the living creature to a subsequent contact stimulus.

Take a typical case. An out-fielder reaches up and catches a batted ball. Leaving for a moment the interpretation, what is the essential fact which we all recognize? It is that the player consciously *anticipates* the outcome of events. He begins to react to the contact stimulus before the contact stimulus occurs. In plain language, he foresees the contact. But foresight is not limited to man. Looking down the organic scale we find everywhere, even in exceedingly low forms of life, contrivances of structure and function whereby an animal can anticipate through behavior the outcome of physical events. The dog chasing the hare runs first in one direction, then turns as the hare turns, and so on till he catches his prey. Medusa moves its tentacles toward approaching food and is thereby prepared to draw the nourishing substance into its mouth. In almost every creature results are foreseen—events are prepared for—reactions to stimuli begin before the stimuli actually occur. That is, the same reaction may be a direct response to one sort of stimulus and a teleological anticipation of another. In the case of avoiding-reactions the contact stimulus is prevented altogether by the foresight which the distant-stimuli produce.

Why attribute the purposive reactions in man to a special 'prophetic insight' of consciousness, when the predisposition can be amply explained by the mechanism of nervous coördination? Every distant-stimulus furnishes anticipations of a possible future complex experience; it yields certain parts of the complex before the remaining parts present themselves. In the example cited the fielder sees the ball and begins to react before it touches his hand; here the visual stimulus outstrips the contact stimulus. When we see an axe strike a distant tree we wink before hearing the blow; in this case one distant-stimulus outstrips another. There is nothing hyperphysical in a dislocation of the order of sensations.

One complication calls for special consideration,—namely, when ideal factors enter into the situation; that is, when part of the experience is due to central stimulation. Psychological studies of association indicate how ideas are aroused by sensations; on the nervous side this means that higher or ideal processes are stimulated by lower or sensory processes. Either ideal or sensory activity may issue in a reaction through motor discharge. It follows, then, that a distant-stimulus may excite a sensory brain process, that this may lead to an ideal brain process, and that the latter may thereupon produce a reaction adapted to a future contact stimulus or to another distant-stimulus.

When we see the axe descend for perhaps the twentieth time, we 'attend' to the coming sound. When the fielder raises his hand to catch the ball he reacts to the visual stimulus plus the ideal data organized through repeated ideomotor experience. If past stimuli leave any trace in the nerve centers (any *engram*, to use Semon's term) the reaction may follow the restimulation of that trace as readily as it will follow a direct sensory stimulus. Take a more complex case. I start for the station to board a train; that is, there arises in my *present* consciousness the mental image of boarding the train or the image of some more remote activity, accompanied by an emotional state of considerable strength. The brain process which is the outer aspect of this very complex mental process issues step by step in reactions which anticipate the successive environmental situations.

Mere complexity should not becloud the issue. Every brain process, like every reflex activity, is presumably the result of physicochemical processes. The assumption of a mysterious intuition or 'psychic force' adds nothing to the mechanistic explanation, even when the latter is most fragmentary. Professor Minot and the interactionists go out of their way unnecessarily in assuming a special activity of consciousness to account for the dislocation of reactions from sensations. The nervous organization suffices to explain it. Distant-stimuli and central stimuli coöperate to bring about anticipatory reactions; foresight is but the conscious side of

this process. The phenomenon is *both* physical and mental. Foresight is no more a special perquisite of the mind than hindsight.

The genesis of purpose is explicable on scientific and mechanistic grounds by natural selection.[1] The old anthropomorphic teleology may be replaced by a natural teleology. The neural basis for anticipatory reactions originated through fortuitous variations; such reactions proved beneficial to the organism and were selected. Those creatures which are able to anticipate the outcome of events have a distinct advantage in the struggle for existence; those which fail to anticipate are most likely to vanish from the earth. It matters not whether the basis be inherited structure, as in the case of instinct, or adaptive function, as in trial and error learning. The purposive activity of the living organism may be adequately accounted for in objective terms, without an appeal to special attributes of consciousness. Natural teleology is a scientific corollary to natural selection.

If vital and mental phenomena are reducible to physicochemical terms, it does not follow that biology and psychology are any the less independent sciences. The very same occurrence may have several totally different meanings. Let me illustrate. A commander of infantry shouts, 'Charge!' and the whole company dash forward. In one sense the captain's cry is a mere matter of *acoustics* and the motion of his men a problem in *mechanics*. A full description of the occurrence will include also the *chemistry* of muscle. But to stop here in our explanation would be absurd. Biology very properly regards the organism as a unit; the *physiology* of vocalization and locomotion are genuine scientific problems. The same events take on a *psychological* meaning when treated as 'call' and 'response.' And again, as an instance of human interaction they belong to the province of *sociology*. Furthermore, if the charge takes place on a battlefield it may present a problem in *military science*, a problem in *economics*, and a problem in *jurisprudence*. The physicochemical description

[1] "Teleology, then, when brought to its stronghold, is a genetic outcome" (J. M. Baldwin, 'Development and Evolution,' p. 279).

merely explains the raw material; it does not touch the sociological situation at all. When an acoustic vibration generated in the larynx is regarded as a social signal, the elementary facts are grouped into higher complexes, and such synthetic units require a totally different description. Baldwin points out that these higher syntheses are new *modes* which reality acquires in the course of evolution.[1]

Consciousness and Behavior

That primitive notions of choice, reasoning, and foresight persist today among scientists, is largely due to the interaction theory and vitalism. The regularity of physical nature is too well understood to admit of anything but a mechanistic view of the material world. A vitalistic or idea-force hypothesis involves the assumption that non-physical forces modify physical events. Consciousness is the most obvious non-physical datum; and so the interaction hypothesis is adopted. The psychologist's analysis and synthesis of consciousness has not sufficed to clear away the popular misconceptions regarding mental phenomena. Even today many intelligent thinkers believe in occult mental influences, and base their world-view on the Mysterious.

Psychology itself is in a measure responsible for this. Our science has been too much dominated by introspective analysis. We need a more thorough investigation of the objective side of experience. Such writers as Watson[2] and Bechterew[3] would remedy the defect by abandoning altogether the study of consciousness. While we may not agree with Professor Watson's destructive critique, his arguments in behalf of the science of behavior are convincing.

Introspection needs checking from outside sources. Our so-called sensations are by no means pure sense data; they are

[1] Baldwin, 'The Origin of a "Thing" and its Nature,' Psychol. Rev., 1895, *2*, 551–573; 'The Theory of Genetic Modes,' Development and Evolution, 300–334. A later writer has elaborated Baldwin's view under the name of 'creative evolution.'

[2] J. B. Watson, 'Psychology as the Behaviorist Views It,' Psychol. Rev., 1913, *20*, 158–177; 'Image and Affection in Behavior,' *J. of Phil., Psychol., &c.*, 1913, *10*, 421–428.

[3] W. von Bechterew, Objective Psychologie, Leipzig: Teubner, 1913.

resultants of sense stimuli and internal stimuli working together. Few of us can effectively analyze our most common experiences. In the classic example of perceiving an orange it is by no means easy to pick out the simple sensations of color, odor, weight, etc. A taste factor is generally present in our perception of the untasted orange, and a systemic attribute of pleasure tinges the whole experience. The psychological dispute as to whether some 150 elementary color sensations exist or merely 3 or 4, illustrates the difficulty of separating by introspection the central from the sensory data in comparatively simple experiences. The recent debate on imageless thought is another instance in point.

The biologist may find his eye or his microscope a defective instrument of research, but at least he can determine within narrow limits the direction and amount of error. Introspection on the contrary is ever liable to confuse peripheral with central factors, whether in perception or in ideation, and only the careful training of a Titchener can avoid hopeless confusion. Mental phenomena are not more obscure and more uncertain than physiological phenomena. The fault lies rather in our complacent reliance on introspection. Despite James's warning most of us are apt to forget, when we analyze our own consciousness, that the analysis itself introduces new factors into the datum of observation. On this account, if on no other, we must check up our introspective results by objective study.

But there is a reason still more cogent for the study of behavior. Objective methods are essential to genetic investigation. Animal psychology is more hindered than helped by introspective interpretation. The proto-esthesia of amœba is radically different from any human experience. To interpret amœba's activity in terms of human *consciousness* throws no light on either consciousness or behavior. On the other hand, the study of amœba's *behavior* throws considerable light on the behavior of higher organisms, as Jennings and Loeb have shown. An examination of the gradual evolution of tropisms, reflexes, instincts, intelligent action, and rational volition enables us to understand far better than before the meaning of human acts and consciousness.

The hope of psychology in the near future seems to lie in the study of behavior. Behavior reveals the dynamic aspect more fully than introspection. Introspective psychology is largely a Linnæan system. It deals with established varieties of experience. Behavior psychology is Darwinian. It traces the growth of experience in the animal series. It supplies material for the study of mental growth in the child.

An autocrat of all the sciences might well decree that psychologists give up introspective study for a term of years and devote themselves to the investigation of behavior. Indeed, a beneficent tyrant might properly go further. The psychologist today needs a substantial grounding in physiology and biochemistry. To understand behavior we must understand the vital processes; and these in turn demand a knowledge of carbon compounds and colloid phenomena which few psychologists possess. How can we interpret attention or inhibition without a knowledge of physiology? What light is thrown on effort and fatigue by an understanding of the chemical changes in muscle! According to Huxley, "Psychology is inseparably linked with physiology."[1] Today he would doubtless add that physiology is inseparably linked with chemistry.

The double-aspect view welcomes the study of behavior, with its physiological implications. But it does not limit psychology to that side alone. Introspection is a legitimate field for exact investigation. With all its shortcomings, modern psychology has yielded many results of scientific worth. The volumes of James and Stout, of Wundt and Titchener, are prime contributions to science. In Part IV. of his 'Psychology,' Professor Yerkes brings together a striking list of psychological laws. Many of these generalizations would be ruled out altogether under Professor Watson's conception of the scope of psychology. We surely all agree that any *exact generalization* belongs to the domain of science. Introspection has already yielded exact results; an impartial tribunal should accept its credentials.[2]

[1] T. H. Huxley, Ency. Britan., 9th ed., Art. 'Biology,' Vol. 3, p. 679.

[2] See, among others, J. R. Angell's recent defense of 'subjective' psychology (Psychol. Rev., 1913, *20*, 255–270).

One question remains to be considered. If consciousness and behavior are both legitimate subjects for scientific investigation, do they constitute a single science? Or shall we distinguish between the science of consciousness and the science of behavior? Something may be said for either attitude. The dividing line between kindred sciences is vague and their demarcation is largely a matter of convenience. The double-aspect view would prefer to treat conscious processes and behavior processes as branches of a single science, inasmuch as they are two phases of the same events.[1]

This involves a new definition of psychology. We can no longer regard it as the science of conscious phenomena. On the other hand to define it with Watson[2] as the science of behavior, or with Pillsbury[3] and Meyer[4] as the science of *human* behavior, cuts us off altogether from the study of introspective data; it flings the whole classic psychology overboard. Professor Pillsbury's volume is all that the most thoroughgoing introspectionist could desire. But to accomplish this he makes human behavior synonymous with consciousness.[5] Professor Meyer, on the contrary, uses introspective data only to arrive at laws of nervous activity and behavior.[6]

A broader concept must be had if both aspects, behavior and consciousness, are to be included in psychology. Professor Marvin regards psychology as the study of 'that which controls reactions,'[7] but he makes no provision for the mental processes which accompany brain activity. Professor Dunlap

[1] In the Psychol. Bull., 1906, *3*, 217–228, the present writer developed a classification of mental 'functions' on different lines from the traditional classes of 'structure elements.' It is there pointed out that 'quality change' which earlier writers call 'mental synthesis' is an important factor in consciousness, while in the physical world differences in quality vanish one by one as science examines the phenomena more closely.

[2] J. B. Watson, 'Psychology as the Behaviorist Views It,' Psychol. Rev., 1913, *20*, 158.

[3] W. B. Pillsbury, 'Essentials of Psychology,' p. 1.

[4] Max Meyer, 'The Fundamental Laws of Human Behavior.' His definition is implied in the title of the book.

[5] This is true also of W. McDougall, who defines psychology as 'the positive science of behavior,' 'Psychology' (Home Univ. Library), p. 38.

[6] See his articles in *J. of Phil., Psychol. &c.*, 1912, *9*, 365–371, and *Amer. J. of Psychol.*, 1913, *24*, 554–563.

[7] W. T. Marvin, 'First Book in Metaphysics,' p. 259.

defines psychology as *the study of experience*,[1] but unfortunately he restricts the meaning of 'experience' to 'consciousness.'[2] Of all available terms, 'experience' requires least change in connotation to cover both consciousness and behavior. Using 'experience' in this broad sense we reach a concise working definition: *Psychology is the science of individual experience.*

Experience implies something which comes to the individual from without; and this is precisely what the larger psychology investigates. Behavior is distinct from physiology. Behavior psychology is the science of those organic processes which bring the individual into relation with his environment, while physiology is the science of the organic processes as such. The physiological functions of nutrition, growth, regulation, reproduction, are distinct from the psychological functions of sensation, discrimination, language, reasoning. Carried over to consciousness the relation of organism to environment becomes the relation of self to not-self. All the phenomena of consciousness, save perhaps certain obscure feelings, are concerned with the relations of self to environment. The exception disappears if we assume that the feeling-tone of consciousness brings the apperceptive self into relation with the lower selves or lower centers of the organism. From the double-aspect standpoint, then, we may define psychology as *the science of the individual organism or consciousness, as related to its environment.*[3]

To sum up: Science is not yet ready to adopt a metaphysics of mind and matter. But some working hypothesis of the psychoneural relation is needed in order to fix the scientific status of psychology. The double-aspect view (monodualism) seems to fit the conditions best. This conception of the relationship between mental and physical becomes clear when we examine the analogous relation between surface and mass in our perception of material phenomena.

[1] K. Dunlap, 'System of Psychology,' p. 4.

[2] *Op. cit.*, p. 6.

[3] Under this definition the data of psychology may be studied in both ways; e.g., discrimination is both *selective reaction* and *sensation-difference*, language is both *expression* and *communication*.

If mental and physical activity are two inseparable aspects of one series of events, then the scientific assumption of uniformity or 'law' is extended from the physical into the mental sphere. The old anthropomorphic conception of choice and reason must be radically amended. In the light of modern science the presumption is that mental phenomena, including choice and reason, are as uniform as physical events. The burden of proof rests on those who deny the regularity and determinacy of human volition and human reasoning. Even teleology may be brought into line with the mechanistic processes of nature. Foresight is the conscious counterpart of purposive activity, and purposive activity is due to distant-stimuli which prepare responses to subsequent contact stimuli by means of a complex nervous mechanism; its beginnings are manifest far down the organic scale.

Psychology should embrace both the inner and outer aspects of experience. It is the science of the relations between the individual and his environment. These relations may be studied either objectively as behavior, or introspectively as events of consciousness. Behavior study is essential to an understanding of genetic problems; it serves also to check up the data of introspection. Introspective psychology has disclosed uniformities among mental events; it claims scientific recognition as a branch which contributes to a unified view of the world. Without consciousness there would be no scientific observation or generalization. Sense perception and the logical processes require analysis quite as much as the facts and values which they reveal. Science must study its instruments as well as its data.

THE THEORY OF TWO FACTORS[1]

BY C. SPEARMAN

CONTENTS

1. *Object of the Paper.*—For the increasingly numerous workers in the field of individual psychology, it is always a notable event when a new investigation, such as Simpson's 'Correlations of Mental Abilities,' issues from the laboratory directed by Professor Thorndike.[2] The investigation is on the now familiar basis of mental tests and correlational coefficients. But the tests present the novel and interesting feature of having been applied to adults whose intelligence had been demonstrated by the sternest criterion, the struggle of life. These subjects included, on the one hand, an intellectual *élite* of 17 professors and advanced students of Columbia University and, on the other, a proletariat of 20 persons 'selected from men in New York City who had never held any position demanding a high grade of intelligence.'

The publication seems to have been made almost simultaneously with one by Dr. Hart and myself,[3] so that neither was able to profit by the other. This was unfortunate, since the two researches form essential complements to one another,

[1] From the Psychological Laboratory, University College, University of London.

[2] Columbia University Contributions to Education, Teachers College Series, No. 53, 1912.

[3] 'General Ability, its Existence and Nature,' *British Journal of Psychology*, Vol. V., p. 53, 1912.

the main value of Simpson's work lying in the empirical facts elicited, while that of the other consists in an improved method of treating such facts.

The present paper proposes to make good this deficiency. Simpson's data have been elaborated by the improved method. As will be seen, the not inconsiderable labor expended has met with its reward; it appears to have resolved a disquieting scientific discord into firm harmony.

2. *Data of Simpson and Thorndike.*—Their correlations are given in the following table. They were calculated by the ordinary Bravais-Pearson formula, and have not (in this table) been corrected for 'attenuation' or otherwise altered.

TABLE I

THE SIMPSON-THORNDIKE CORRELATIONS ('RAW')[1]

	Ebbinghaus Test	Hard Opposites	Memory of Words	Easy Opposites	*A* Test	Memory of Passages	Adding	Geometrical Forms	Learning Pairs	Recognizing Forms	Scroll	Completing Words	Estimating Lengths	Drawing Lengths
	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1. Ebbinghaus test........	...	98	94	79	62	91	71	54	78	88	55	42	33	25
2. Hard opposites..........	98	...	84	80	64	81	79	70	73	74	52	43	26	25
3. Memory of words	94	84	...	62	55	82	49	56	73	71	53	40	28	21
4. Easy opposites.........	79	80	62	...	57	52	68	53	42	56	45	29	38	48
5. *A* test..................	62	64	55	57	...	55	54	73	39	51	39	59	25	22
6. Memory of passages	91	81	82	52	55	...	53	57	59	66	54	31	28	19
7. Adding.................	71	79	49	68	54	53	...	45	39	47	51	57	17	25
8. Geometrical forms......	54	70	56	53	73	57	45	...	35	49	54	56	25	25
9. Learning pairs..........	78	73	73	42	39	59	39	35	...	69	36	29	26	09
10. Recognizing forms......	88	74	71	56	51	66	47	49	69	...	44	37	34	28
11. Scroll.................	55	52	53	45	39	54	51	34	36	44	...	31	19	27
12. Completing words......	42	43	40	29	59	31	57	56	29	37	31	...	21	07
13. Estimating lengths	33	26	28	38	25	28	17	25	26	34	19	21	...	24
14. Drawing lengths........	25	25	21	48	22	19	25	25	09	28	27	07	24	...

Most of the tests used were already well known and may be found in Whipple's Manual.[2] The 'Ebbinghaus,' 'hard opposites,' and 'easy opposites' are familiar to every psychologist.[3] 'Memory of words,' 'memory of passages,' and 'learning pairs' are memorizing performances. The '*A* test' and the

[1] *Ibidem*, p. 56.

[2] 'Manual of Mental and Physical Tests,' 1910.

[3] See Whipple, pp. 445 and 319.

'geometrical figures' are both what are usually known as 'cancellation tests.'[1] Nos. 13 and 14 both consist in discrimination of length of visual lines, but 13 uses the psychophysical method of 'reproduction,' whereas 14 apparently uses that of 'minimal changes.' The remaining titles, 'recognizing forms,' 'completing words,' and 'adding' sufficiently explain themselves.

3. *Theory of Two Factors.*—In evaluating the above table, it must be remembered how little scientific significance usually attaches to any single correlational coefficient considered by itself. Such a correlation almost always admits of indefinitely numerous interpretations. To eliminate this equivocality and penetrate down to the underlying truths, every correlation needs to be regarded in the light of all the others. Not the single correlations, but their inter-relation, is the vital matter. Simpson and Thorndike seem to have had this in mind, since they principally seek to verify a recent theory which claims to have discovered this inter-relation in the case of mental tests.

This theory is to the effect that all mental tests exhibit correlation additional to, and usually much larger than, that arising from merely casual causes, such as similarity between one test and another. This additional and larger correlation is asserted to be deducible from one of the oldest, most widely held, though, indeed, obscurely enough uttered hypothesis of psychology: the hypothesis, *that all the intellective activity of any person depends in some degree on one and the same general fund of mental energy.*[2]

That correlation of merely casual origin should be thus excepted is clearly indispensable. Without this proviso, nobody would be foolish enough to assign any fixed value to the amount of correlation between two tests. For by making these sufficiently alike, in other words, by sufficiently increasing

[1] See Whipple, p. 254.

[2] Strictly speaking, the theory is broader. It only maintains the existence of *some* general factor (or system of factors). But as this is believed to consist mainly in a general fund of energy, we will for greater concreteness and lucidity speak only of this energy in the present paper.

the elements common to the two, the correlation between them could obviously be raised to any extent whatever.[1]

Still, in practice the limitation of the theory by this proviso is far less than might have been expected, as will readily be seen by examining it in the case of Table I. The table happens to contain one of the few instances where previous investigations have found this casual correlation to be large; this is, between the two tests of 'cancellation,' Nos. 5 and 8, which differ solely in the shape and familiarity of the figures cancelled.[2] To agree with the theory, therefore, the correlation between these two tests should *not* be readily explicable by the general energy, but should be appreciably greater than required by the latter. Another case possibly concerned is that of the three memorizing tests; in previous investigations, somewhat similar performances have been occasionally reported to exhibit correlations with one another a little larger than was to be expected from the influence of the general energy alone.[3] The last and still more dubious case is furnished by the two discriminations of length. At first sight, these might well seem so much alike as almost to constitute the same test. But previous experiment has indicated that sensory discrimination is extremely dependent on the psychophysical method used, and is especially dominated by the vagaries of the method of 'reproduction.'[4] It remains an open question, therefore, whether the difference of method suffices to preclude any special correlation arising from the resemblance. Thus, out of all the 91 different correlations in Table I., only one ought certainly to be affected by the proviso, while four others have some moderate probability of being so.

The statement of the theory on page 113 contains another important qualification; the success of any intellectual per-

[1] It is hard to find excuse for many critics of the theory having completely overlooked this proviso. Not only was it explicitly stated, but several actual instances were adduced to illustrate and prove it (*B. Journ. Psych.*, V., p. 59). That common elements can produce special correlation is as undeniable as that they can cause 'transference of training'; the two cases are closely akin.

[2] The previous instance of two tests differing in this manner is described in *Brit. J. Psych.*, V., p. 73.

[3] *Brit. J. Psych.*, IV., p. 293; V., p. 75.

[4] *B. J. P.*, V., p. 74.

formance is said to depend on the general energy *in some degree* only. This indicates that there is a *second* factor in the person's success, namely, his *specific capacity for that particular kind of performance.* Hence, Professor Sancti de Sanctis has appropriately termed the theory that of 'two factors.'[1] In fact, it is just this doubleness of dependence—say the supporters of the theory—that has so long confused psychologists as to the real state of affairs.

The double relation may be found clearer by many readers when expressed physiologically. And, of course, the accord with physiology is itself an important evidence. According to the commonly accepted theory of cerebral localization of function, every mental performance involves an activity of some particular group of cortical neurons. To this the present theory adds, that the particular group of neurons needs more or less reinforcement by the energy of the whole cortex (or some even more extensive area).[2] Thus, the two factors in success are quite distinct; firstly, there is the state of the particular group of neurons, their development and organization; and secondly, there is the whole cortex. The former may be called the 'specific' factor, as it is specific to that particular performance. The latter constitutes the 'general' factor, since it is required for all performances.

As regards the correlations between mental tests, the theory of two factors maintains that the specific one merely contributes the usually insignificant 'casual' additions discussed on pages 103 and 104. With this small exception, the whole of the correlations between the tests is declared to be deducible from the general factor alone.[3]

4. *Older Methods of Verifying this Theory.*—Now, the paper on 'General Ability' quoted on page 101 showed that the current attempts to verify this theory on the actually observed facts suffered from grave defects.

[1] See Proc. of International Congress of Medicine, Section for Psychiatry, London, 1913; discussion on the paper of Hart and Spearman on 'Dementia.'

[2] It is impossible to enter here into the precise physiological mode of action of this general energy; but it does not appear to offer serious difficulties.

[3] Of course, this proposition—and, indeed, the whole paper—only applies immediately to ordinary tests of mental ability. It does not claim, for instance, to include correlations between tests of fatiguability.

These attempts were centered on the proposition that, if the correlations were really all due to one and the same factor, they should admit of being arranged 'hierarchically'; this means, in such a manner that the values are highest in one corner of the table (generally, the left top is chosen) and thence gradually diminish in both vertical and horizontal directions, the diminution of every column (or row) being in the same proportion.[1]

But this criterion of hierarchy has turned out a failure. Not, indeed, that it has been convicted of any error. But it has shown itself singularly open to arbitrary usage. Its essential fault lies in being strictly applicable only to cases where the hierarchy may be expected to hold good with absolute exactness. The decision as to whether any table of correlations *tends* towards permitting the hierarchical arrangement is left a matter of dogmatic assertion; those writers who are biased in favor of the theory always find such a tendency to exist; whereas those with the contrary bias invariably arrive at the contrary conclusion. For an instance, we need not go beyond our Table I.; here Thorndike and Simpson discover no such tendency towards admitting of hierarchical arrangement; on the other hand, to the present writer the tendency is clearly present.

This fault is the more disastrous, since an exact hierarchy—or, indeed, any other perfectly regular arrangement—cannot be expected under any circumstances whatever. Every actual measurement, psychical or physical, is liable to more or less error; and ordinary scientific measurements, where a limited number of persons or objects are taken as a representative sample of the entire class, are especially disturbed by the 'error of sampling' (the median magnitude of this disturbance is, as is well known, expressed by its 'probable error').

Occasionally, other methods besides the hierarchy have been employed. Some particular correlations have been selected from the table for combination into a mathematical function, and then the latter's value has been compared with

[1] For the original statement of this proposition, see Spearman, 'General Intelligence,' *Am. J. Psych.*, XV., 1904, p. 274.

that to be expected from the theory. But these attempts have been, if possible, even worse off; for they, too, have for the most part ignored the errors of sampling; in fact, the sampling error of the function would usually have been exceedingly difficult to determine. And in addition, these other methods have had the grave vice of depending on certain arbitrarily selected values, instead of resting fairly on the entire available data.

5. *Improved Method.*—The last two sections have indicated some of the misunderstandings of theory and difficulties of method with which Simpson and Thorndike had to contend, and through which their valuable work has so far been deprived of valid interpretation. Let us turn to the improved method offered by the paper on 'General Ability.'

In the first place, the consequences flowing from the theory of two factors have been deduced in a far more satisfactory manner than previously. The following argument, besides being mathematically rigorous, is simple enough for a schoolboy to understand.

Let a, b, p, and q denote any four mental tests, and g the hypothetical general fund of energy. Using the usual symbols, let r_{ap} denote the correlation between a and p, while r_{aq}, r_{bp}, r_{bq}, r_{ag}, and r_{pg} have similar meanings. And let $r_{ap\,.\,g}$ denote the correlation that a would have with p if the influence of the variations of g were eliminated. We have, then, by Yule's well known formula for 'partial coefficients':

$$r_{ap\,.\,g} = \frac{r_{ap} - r_{ag} \cdot r_{bg}}{\sqrt{1 - r^2_{ag}}\,\sqrt{1 - r^2_{bg}}} \qquad (A)^1$$

But by the theory, the correlation between a and p is wholly due to g, so that when r_{ap} is freed from the influence of g, the ensuing value $r_{ap\,.\,g}$ must $= 0$; hence, by equation (A), $r_{ag} \cdot r_{pg} = r_{ap}$; similarly, $r_{bg} \cdot r_{pg} = r_{bp}$. Dividing the former equation by the latter, we get $r_{ag}/r_{bg} = r_{ap}/r_{bp}$, and similarly $= r_{aq}/r_{bq}$. Whence we arrive at the fundamental equation:

$$\frac{r_{ap}}{r_{aq}} = \frac{r_{bp}}{r_{bq}} \qquad (B)$$

[1] Udny Yule, 'Introduction to the Theory of Statistics,' p. 235.

Here, the hypothetical quantity g has been eliminated, and there remains a relation asserted to hold good—when all due allowances have been made—of the correlations between any four tests, a, b, p, and q. The proof is so short and clear, and the resulting relation is so astonishingly simple and definite, that the theory now at any rate possesses one of the most valuable characteristics in highest degree: the capability of being readily submitted to crucial quantitative verification.

Also, equation (B) is clearly consonant with the older criterion of 'hierarchy,' for when the equation holds good, and then alone, the table of correlations admits of arrangement in the hierarchical order.[1]

The equation may be illustrated from Table I. The symbols a and b may, for example, be taken as denoting our tests 2 and 9. Let p and q denote any pair out of the remaining tests. If the equation holds good, the correlations of 2 with any other two tests should be in the same proportion to one another as the correlations of 9 with the same two tests. All the correlations in question are reproduced in Table II.

TABLE II

		Test 2, Denoted by a in Equation (B)	Test 9, Denoted by b in Equation (B)
Test 1	denoted by p and q in equation (B)	98	78
" 3		84	73
" 4		80	42
" 5		64	39
" 6		81	59
" 7		79	39
" 8		70	35
" 10		74	69
" 11		52	36
" 12		43	29
" 13		26	26
" 14		25	9

The next step in our improvement is—*exact* fulfilment of the equation being *a priori* precluded by sampling errors and perhaps by casual correlation—to devise means of measuring the *general tendency* of Table II. to satisfy the equation.

[1] This is too obvious to need formal demonstration.

This purpose is served by the following reasoning. As we have just seen, in order that Table II. should satisfy equation (B), any two values under test 2 must bear the same proportion to one another as the two corresponding values under test 9. But this is equivalent to saying that the whole column under test 2 must be perfectly correlated with that under 9. If, on the other hand, the values in Table II. have no tendency whatever to satisfy equation (B), then the correlation between columns 2 and 9 must be zero.[1] *Hence, the required measure of the tendency to fulfil the equation* (B) *is given by the correlation between the two columns of correlations.* Of course, this is calculated just as easily as between any other two series of numbers. We may conveniently denote it by the symbol R_{ab}.[2] It turns out to have here the positive and extremely high value of $+.84$.[3]

Clearly, this value will remain unaltered by any, however arbitrary, rearrangement of the order of the tests in table, and it thus escapes a grave defect in the hierarchical method.

[1] A similar result ensues from the not unplausible hypothesis, that each performance depends on a randomly selected group of very numerous independent elements, and that the correlation between any two performances is due to some of the elements happening to be common to both groups. For it could easily be shown that under these assumptions the correlation (compensated for sampling errors) between any two columns will tend to equal the correlation (*un*corrected for attenuation) between the two performances from which the columns derive; in our notation, R'_{ab} will equal r_{ab}; and both will average little over zero.

[2] Formally expressed:

$$R_{ab} = \frac{S(\rho_{xa}\rho_{xb})}{\sqrt{S(\rho^2_{xa})}\sqrt{S(\rho^2_{xb})}}$$

where ρ_{xa} denotes the deviation of any value in the column under test 2 from the average of that column; ρ_{xb} has a similar meaning with regard to the column under test 2; and S denotes the sum of the values in the bracket.

It may be noted in passing, that the significance of R_{ab} is by no means confined to the present problem. In particular, its relations to r_{ab} are most interesting; the one may easily be positive when the other is negative, or vice versa. For many purposes, R_{ab} seems to be the more important value of the two.

[3] The question arises, whether or not the coefficients should be corrected for 'attenuation' before calculating R_{ab}. The matter is simplified by the easily demonstrable fact, that if R_{ab} is $= 1$ for the corrected coefficients, it must be so for the uncorrected ones also, and vice versa. And even if R_{ab} has any other value, it will not in general by appreciably modified by correcting for attenuation (the statement sometimes made, that the hierarchy tends to be much better for uncorrected than for corrected coefficients is erroneous). The correcting process has the disadvantage of greatly complicating equation (C); hence, it has been omitted here, as in the paper on 'General Ability.'

The third and final step in the present improvement of method is to compensate for the influence of the errors of sampling. The value taken by R_{ab} when thus compensated may be denoted by R'_{ab}.[1] The detailed calculations for Table II. are given in the appendix, in order to enable any one to make such calculations for himself. The value comes here to no less than +.91.[2]

6. *Examination of the Theory by the Improved Method.*—Let us now apply this new method in an absolutely straightforward manner to the data obtained by Simpson and given in Table I.

Take first the question of 'casual' correlation. We have seen (p. 104) that out of the ninety-one different coefficients in this table only one unquestionably should be too large to agree with equation (B) (or with the hierarchy), namely, that between the two cancellation tests (5 and 8). Four other coefficients, those between the three forms of memorizing (3, 6, and 9) and between the two modes of discriminating length (13 and 14), have some probability of being too large. As the amounts involved are insignificant as compared with the trend of the whole table, we may be allowed to dispose summarily of this small matter by the old rough criterion of hierarchy. In drawing up the table, evidently, some attempt has been made to approximate to the hierarchical arrangement; hence, any values too great to fit into the hierarchy

[1] The formula is:

$$R'_{ab} = \frac{S(\rho_{xa}\rho_{xb}) - (n-1)r_{ab}\overline{\sigma_{xa}\sigma_{xb}}}{\sqrt{S(\rho^2_{xa})-(n-1)\overline{\sigma^2_{xa}}}\ \sqrt{S(\rho^2_{xb}) - (n-1)\overline{\sigma^2_{xb}}}} \qquad (C)$$

where the ρ's have the same denotation as before; $\overline{\sigma^2_{xa}}$ is the mean value of (probable error ÷ .6745)² while $\overline{\sigma^2_{xb}}$ and $\overline{\sigma_{xa}\sigma_{xb}}$ have similar meanings; n is the number of different values of x. For proof, see *B. J. P.*, V., p. 80.

[2] It should be noted that this compensation for the effect of sampling errors, like most corrections, is devised so as to make the result right *on an average*. Consequently, the individual values obtained for R'_{ab} may be expected to fluctuate equally above and below the *true* value (obviously, they must do this, if their mean square deviation from the truth is to be a minimum). In the extreme case that the true value approximates to complete unity, about half the individual values obtained for R'_{ab} ought therefore to be greater than unity, although of course no true value of R_{ab} could be greater.

should be distinguished by being larger than its four neighbors, that is, the four above and below it and to its right and left. Now, the correlation between the cancellations exceeds its neighbors by an average of no less than .22; those between the memorizings do so by .11; while that between the two length tests exceeds its only neighbor by .17.[1] Thus, all the anticipations of the theory are borne out by the actual facts. And I find that the more elaborate demonstration, on the basis of measuring the discrepancies of the coefficients from equation (*B*), only leads to the same result with greater emphasis.

After this confirmation concerning these casual discrepancies to be expected from equation (*B*), let us turn to the more essential matter of the general validity of the equation. To deal with this effectively, it is well first of all to get rid of any excessive masking influence to be expected from the 'casual' correlation. But on page 104, we learn that this can be achieved with tolerable completeness merely by eliminating one of the cancellation tests, or, more fairly, by pooling the two together. We have simply to replace columns and rows 5 and 8 in Table I. by their average. This results in the very slightly modified Table III.

The final and crucial business is to work out the correlation between each pair of columns in Table III., using the formula given in equation (*C*).

The difficulty, however, arises, that it is not always possible to determine this correlation (either R'_{ab} or R_{ab}) satisfactorily. The calculation of a correlation is, as every one knows, based on the deviations of the values in question from their own average. But should these deviations be small and the samp-

[1] This small excess, .17 only, shows that the use of the method of reproduction so dominates the character of test 14 as to render it almost completely different from test 13. This incidentally reconciles an old discrepancy of result; Thorndike, Lay, and Dean found too large a correlation between discrimination of weight and that of length to be attributable to any general factor (*Am. Jour. Psych.*, XX., 364); and this was regarded as arguing against the existence of any such factor, and as indicating instead an especial correlation between all performances belonging to the 'sensory level.' Hart and I suggested, rather, that some specific correlation had been produced between the two sorts of discrimination merely by using in both cases the very dubious method of reproduction. Above the suspicion is justified; this method proves actually to possess such a dominating character; whereas all signs of any 'sensory level' have now vanished.

Table III

The Simpson-Thorndike Correlations after Pooling Together the Two Tests of Cancellation

	Ebbinghaus Test	Hard Opposites	Memory of Words	Easy Opposites	Pooled Cancellation	Memory of Passages	Adding	Learning Pairs	Recognizing Forms	Scroll	Completing Words	Estimating Leng ths	Drawing Lengths
	1	2	3	4	5	6	7	8	9	10	11	12	13
1. Ebbinghaus test	...	98	94	79	58	91	71	78	88	55	42	33	25
2. Hard opposites	98	...	84	80	67	81	79	73	74	52	43	26	25
3. Memory of words	94	84	...	62	55	82	49	73	71	53	40	28	21
4. Easy opposites	79	80	62	...	55	52	68	42	56	45	29	38	48
5. Pooled cancellation	58	67	55	55	...	56	49	37	50	46	57	25	23
6. Memory of passages	91	81	82	52	56	...	53	59	66	54	31	28	19
7. Adding	71	79	49	68	49	53	...	39	47	51	57	17	27
8. Learning pairs	78	73	73	42	37	59	39	...	69	36	29	26	09
9. Recognizing forms	88	74	71	56	59	66	47	69	...	44	37	34	28
10. Scroll	55	52	53	45	46	54	51	36	44	...	31	19	27
11. Completing words	42	43	40	29	57	31	57	29	37	31	...	21	07
12. Estimating lengths	33	26	28	38	25	28	17	26	34	19	21	...	24
13. Drawing lengths	25	25	21	48	23	19	29	09	28	27	07	24	...

ling errors relatively large, the former become swamped in the latter, and the calculation is illusive. In practice, therefore, it is necessary to fix some more or less arbitrary limit, at which R'_{ab} is so much disturbed by the sampling errors that its calculation shall be deemed no longer profitable. In the paper on General Ability, it was accordingly found advisable to reject all pairs of columns, in either of which the sum of the squares of the probable errors exceeded one fourth of the sum of the deviations from the average. If we retain this standard on the present occasion, there turn out to be 23 usable pairs of columns. The correlations, both raw and corrected, for each pair are given in the following table.

7. *Conclusion.*—Thus the mean value of the correlation between columns turns out to be no less than +.96. The discrepancy between this and unity—whether it be attributable to the still uneliminated casual correlation, or to mere chance—is of negligible magnitude.

The net result, therefore, of applying the new exact method in a perfectly plain and impartial manner to the experimental data of Simpson and Thorndike is a complete confirmation of the theory of two factors. The opposite conclusion arrived

Table IV

The Correlations for Each Pair of Columns in Table III

	R_{ab} Raw Correlation: $\frac{S(\rho_{ax}\rho_{bx})}{\sqrt{S(\rho^2_{ax})}\sqrt{S(\rho^2_{bx})}}$	R'_{ab} Correlation Corrected for Sampling Errors: $\frac{S(\rho_{ax}\rho_{bx})-(n-1)r_{ab}\sigma_{ax}\sigma_{bx}}{\sqrt{S(\rho^2_{ax})-(n-1)\sigma^2_{ax}} \times \sqrt{S(\rho^2_{bx})-(n-1)\sigma^2_{bx}}}$
1. Ebbinghaus test and hard opposites	+.94	+ .95
2. " " memory of words	+.85	+ .87
3. " " easy opposites	+.93	+ .98
4. " " memory of passages	+.90	+1.02
5. " " recognizing forms	+.88	+ .96
6. Hard opposites and memory of words	+.96	+ .99
7. " " easy opposites	+.96	+1.00
8. " " memory of passages	+.96	+1.14
9. " " learning pairs	+.67	+ .76
10. " " recognizing forms	+.95	+1.05
11. Memory of words and easy opposites	+.96	+ .92
12. " " memory of passages	+.96	+1.10
13. " " learning pairs	+.73	+ .86
14. " " recognizing forms	+.96	+1.05
15. Easy opposites and memory of passages	+.95	+1.08
16. " " learning pairs	+.70	+ .76
17. " " recognizing forms	+.95	+1.08
18. " " cancellation	+.78	+ .99
19. Memory of passages, learning pairs	+.74	+ .92
20. " " recognizing forms	+.95	+1.08
21. Learning pairs and recognizing forms	+.64	+ .75
22. Cancellation and memory of passages	+.69	+ .80
23. " " recognizing forms	+.73	+ .94
Average	+.86	+ .96[1]

at by these investigators themselves proves to have been merely an illusory product of the older defective methods, and to vanish on removal of the defects.

This ending cannot but be considered highly desirable for psychology. It shows the present data to be in perfect accord with all the other relevant facts hitherto published, whether by authors biased at the time in favor of the theory or against it. When writing 'General Ability,' these facts embraced the work of 14 experimenters on 1,463 men and women, boys and girls, sane and insane; the average correlation between columns throughout these amounted to +.97. During the brief period that has since lapsed, there has been added the research of Wyatt on 75 children, with a mean R'_{ab} of again +.97;[2] also, there is about to appear one by Abelson, where it

[1] As regards many of the values of R'_{ab} being greater than unity, see note 2 to p. 105.

[2] *B. J. Psych.*, VI., 1913, p. 109.

amounts to +1.02 and one by Webb, where it again comes to +1.02. Of contrary evidence, on the other hand, there has not ever been found to my knowledge one single instance.

The importance of this result is estimated by the present writer very highly. The bare fact of such an exact concordance throughout such a vast extent of independent research, including work of the standard of Thorndike's laboratory, seems to indicate that the science of psychology is entering on a new era. And the conclusion backed by such an accumulated mass of evidence, the theory of two factors, does not confine its significance to individual differences of ability, but —as the writer hopes to show shortly—reaches the heart of all the great problems of attention, association, fatigue, transfer of training, and mental growth. Its bearings are not even limited to the cognitive aspects of mind, but extend their ramifications deep into the affective and conative aspects also.

Appendix

The calculation of R_{ab} and R'_{ab}, exemplified in columns 2 and 9 in Table I.

Tests 2 and 9 are denoted by *a* and *b*. Their correlations with each of the other tests are written in the usual way as

Table V

Values Obtained from Columns 2 and 9 in Table I. for the Purpose of Calculating R_{ab} and R'_{ab}

1	2	3	4	5	6	7	8	9	10	11	12
r_{ax}	r_{bx}	p_{ax}	p_{bx}	e_{ax}	e_{bx}	p^2_{ax}	p^2_{bx}	$p_{ax}p_{bx}$	e^2_{ax}	e^2_{bx}	$e_{ax}e_{bx}$
.98	.78	.33	.33	.01	.04	.1089	.1089	.1089	.0001	.0016	.0004
.84	.73	.19	.28	.03	.05	.0361	.0784	.0532	.0009	.0025	.0015
.80	.42	.15	−.03	.04	.08	.0225	.0609	−.0045	.0016	.0081	.0036
.64	.39	.01	−.06	.06	.09	.0001	.0036	−.0006	.0036	.0081	.0054
.81	.59	.16	.14	.04	.07	.0256	.0196	.0224	.0016	.0049	.0028
.79	.39	.14	−.06	.04	.09	.0196	.0036	−.0084	.0016	.0081	.0036
.70	.35	.05	−.10	.06	.10	.0025	.0100	−.0050	.0036	.0100	.0060
.74	.69	.09	.24	.05	.06	.0081	.0576	.0216	.0025	.0036	.0030
.52	.36	−.13	−.09	.08	.09	.0169	.0081	.0117	.0064	.0081	.0072
.43	.29	−.22	−.16	.09	.10	.0484	.0256	.0352	.0081	.0100	.0090
.26	.26	−.39	−.19	.10	.10	.1521	.0361	.0741	.0100	.0100	.0100
.25	.09	−.40	−.36	.11	.11	.1600	.1296	.1440	.0121	.0121	.0121
Mean = .65	Mean = .45					Sum = .6008	Sum = .4820	Sum = .4526	Mean = .00434	Mean = .00726	Mean = .00538

r_{ax} and r_{bx}, and are given below in columns 1 and 2 respectively of Table V.[1] The deviation of each value of r_{ax} from the mean of r_{ax} is denoted by ρ_{ax} and given below in column 3; ρ_{bx} is analogous, and given in column 4. The 'probable errors' of r_{ax} and r_{bx} are denoted by e_{ax} and e_{bx}; they are given in columns 5 and 6. n is the number of different values of x.

$S(\rho^2_{ax})$, $S(\rho^2_{bx})$ and $S(\rho_{ax}\rho_{bx})$ denote respectively the sums of: the squares of ρ_{ax}, those of ρ_{bx}, and the products $\rho_{ax}\rho_{bx}$. They are given at the bottom of columns 7, 8 and 9. $\overline{\sigma^2_{ax}}$, $\overline{\sigma^2_{bx}}$, and $\overline{\sigma_{ax}\sigma_{bx}}$ denote respectively the mean values of e^2_{ax}, e^2_{bx} and $e_{ax}e_{bx}$, each divided by $.6745^2$; these mean values (anterior to the division by $.6745^2$) are given at the bottom of columns 10, 11, and 12.

Then, by the ordinary correlational formula, the correlation between columns 1 and 2,

$$R_{ab} = \frac{S(\rho_{ax}\rho_{bx})}{\sqrt{S(\rho^2_{ax})}\,\sqrt{S(\rho^2_{bx})}} = \frac{.4526}{\sqrt{.6008}\,\sqrt{.4820}} = .84$$

On correction for sampling errors, the correlation between the same two columns becomes (*B. J. P.*, V., p. 82),

$$R'_{ab} = \frac{S(\rho_{ax}\rho_{bx}) - (n-1)r_{ab}\overline{\sigma_{ax}\sigma_{bx}}}{\sqrt{S(\rho^2_{ax}) - (n-1)\overline{\sigma^2_{ax}}}\,\sqrt{S(\rho^2_{bx}) - (n-1)\overline{\sigma^2_{bx}}}}$$

$$= \frac{.4526 - 11 \times .73 \times \dfrac{.00538}{.6745^2}}{\sqrt{.6008 - 11 \times \dfrac{.00434}{.6745^2}}\,\sqrt{.4820 - 11 \times \dfrac{.00726}{.6745^2}}} = .91.$$

[1] Omitting the two which have no corresponding correlation in the other column.

ON THE USE OF THE ROTATING SECTOR IN PHOTOMETRY

BY A. H. PFUND

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The rotating sector or episcotister has been so widely used in photometric measurements that the soundness of such application has been doubted no more than that of the law of inverse squares. It was somewhat surprising, therefore, that Parker and Patten[1] should have come to the conclusion that errors as large as 5.9 per cent. are introduced when a rotating sector is used to cut down the intensity of a beam of light. Were this instrument of little importance, the above paper might be passed over without comment. However, since we have nothing to take the place of the rotating sector, which is invaluable in photometry, it seems worth while to call attention to the experiments which justify the use of this instrument and also to point out the experimental errors which are responsible for the results of Parker and Patten.

The subject is an old one and deals with the verification of Talbot's[2] law which has been stated by Helmholtz[3] as follows: "If any part of the retina is exited with intermittent light recurring periodically and regularly in the same way, and if the period is sufficiently short, a continuous impression will result, which is the same as that which would result if the total light received in each period were uniformly distributed throughout the whole period." The experimental verification of this law has been carried out by a number of investigators.[4] As the work of Hyde[5] is the most thorough, a brief description of the

[1] Parker and Patten, *Am. Jour. Physiology*, 31, No. 1 (1912).

[2] Talbot, *Phil. Mag.*, Series 3, Vol. 5, p. 321 (1834).

[3] 'Physiolog. Optik.,' II. Auflage, p. 483.

[5] Plateau, *Pogg. Annalen*, 35, p. 457 (1835); Helmholtz, *loc. cit.*; Kleiner, *Pflügers Archiv*, 18, p. 542 (1878); Wiedemann u misserschmidt, *Wied. Annalen*, 34, p. 465 (1888); Lummer u. Brodhun, *Zs. für Instr.-kunde*, 16, p. 299 (1896).

[4] Hyde, Bull. Bureau of Standards, Vol. II., p. 1 (1906).

principle of his method will be given. Two similar Nernst lamps are mounted on the arms of a photometer bench and their distances are so adjusted that a photometric balance is established in a Lummer-Brodhun photometer. If now the light from one of the sources be cut down to ¼ of its original intensity by means of a rotating sector it will be necessary to double *exactly* the distance of the second source in order that a photometric balance be retained, *i. e.*, provided Talbot's law holds. By proceeding in this manner, taking into account a slight correction for the finiteness of the light source, Hyde carried out an exhaustive test of Talbot's Law for white light for all angular openings between 288° and 10° and found that the law was verified to within a possible error of ⅓ of 1 per cent. which probably expresses the limit of accuracy of the experiments. The law was found to hold equally well for red, green and blue light. It is therefore established by methods which are entirely free from objections, that Talbot's law holds to a high degree of accuracy for the human eye.

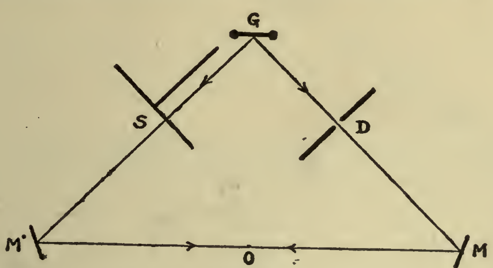

FIG. 1.

The apparatus used by Parker and Patten to test Talbot's law is represented diagrammatically in Fig. 1, where G is a Nernst lamp whose radiations along $GSM'O$ are weakened by a rotating sector S, while those along $GDMO$ are weakened by a diaphragm D. Visual balance of the two beams is first established by means of a Lummer-Brodhun photometer at O and this is subsequently replaced by a radiomicrometer which serves to measure the "physical intensity" of the two beams inividually. If the deflection produced by the energy of the one beam is not equal to that produced by the other then

Talbot's law is supposed to be invalid. Now the principal objection to this method of procedure may be formulated by stating that, while the human eye responds only to the visible radiations, the radiomicrometer responds to the entire range of wave-lengths emitted by the Nernst lamp, *i. e.*, the deflections given by the radiomicrometer are not at all a physical measure of the visible radiations, but of all of the radiations. Considering that the visible radiations form less than 4 per cent. of the total radiant output of a Nernst lamp, it is rather important to see what becomes of the other 96 per cent. which lie in the infra-red and which have by far the greater effect on the radiomicrometer. It is obvious that the diaphragm *D* cuts down the intensity of the light reaching *O* by limiting the effective length of the luminous Nernst lamp glower *G*, as is shown in Fig. 2. If the glower had the same temperature and

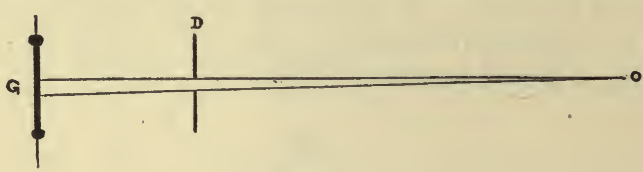

FIG. 2.

hence the same luminosity along its entire length, the method would be sound—but such is not the case. Due to conduction along the lead wires, the temperature and luminosity near the ends of the glower are markedly less than at the centre and as the diapragm limits the effective length of the glower to the central portion, this alone is capable of sending its radiations over the path *GDMO*. If we were to plot the energy curve of the central portion of the glower and also that of an equal length near the end of the glower, we should obtain the curves *A* and *B*, respectively, shown in Fig. 3. Since the maximum of an energy curve is shifted toward the shorter wave-lengths as the temperature of the source is increased, it is obvious that the amount of energy, for curve *A*, lying within the visible spectrum, is the larger. In order to bring about a photometric balance it is necessary to reduce the effective length of the glower by means of the diaphragm until the areas of the two

curves bounded by the confines of the visible spectrum are the same. But in doing this, all ordinates are reduced in the same proportion, as is shown in curve A' which shows the *same* amount of visible radiation as curve B but *less* infra-red. The eye responds only to visible radiations and, according to the eye, equality has been established. The radiomicrometer, however, records deflections which are proportional to the areas $VBRB$ and $VA'RV$. As these areas are different, the deflections will be different even though the amounts of visible radiations be the same.

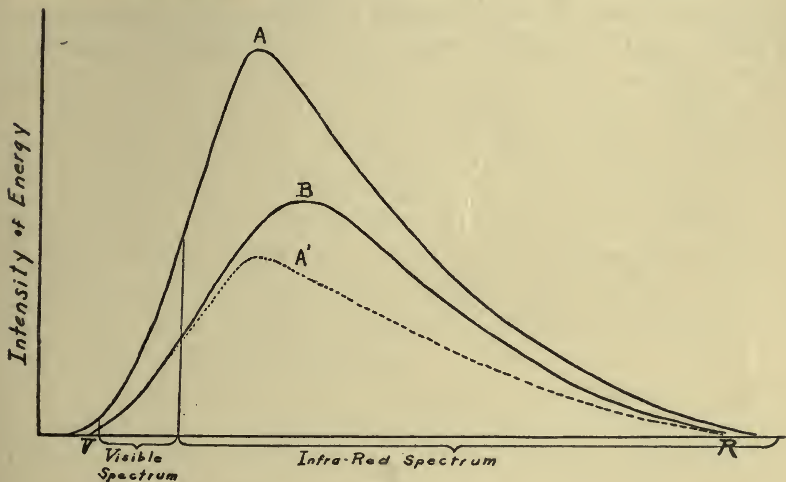

FIG. 3.

This discussion may be applied at once to the method of Parker and Patten. As has been stated, while the diaphragm D permits only the radiations coming from the central or hottest part of the glower to reach O, the rotating sector permits the radiations from the entire glower to pass on. Now the mean temperature of the entire glower is less than that of the central portion, hence, if a visual balance be established, we shall have conditions entirely similar to those shown in Fig. 3 where curve B refers to the energy curve of the radiations transmitted by the rotating sector and curve A', to that of the radiations transmitted through the diaphragm. It is clear that we should expect the radiomicrometer deflections due

to the radiations coming through the diaphragm to be the smaller—which is precisely the result found by Parker and Patten (page 27, *loc. cit.*).

It may likewise be shown with equal ease, that if the width of the diaphragm be so adjusted that the two beams give the same radiomicrometer deflections, then the visual intensity of the beam transmitted by the diaphragm will be the greater (page 28, *loc. cit.*).

In conclusion it may then be stated that the use of the rotating sector has been fully justified by experiments which are entirely free from objections and that the experiments of Parker and Patten in no way cast doubt upon the validity of Talbot's law.

November, 1913.

THE DURATION OF ATTENTION

BY M. LeROY BILLINGS

(From the Psychological Laboratory of the University of Michigan)

Before entering into a discussion of my topic I wish to acknowledge my indebtedness to Professor W. B. Pillsbury, with whom I did this work, to Dr. John F. Shepard for his many helpful suggestions, and to Dr. E. C. Rowe for reading and suggesting changes in the manuscript. I also wish to thank those who acted as subjects for the experiments.

The many estimates in regard to the duration of attention have varied from a few seconds, as given by Professor Pillsbury in his 'Essentials of Psychology,' page 122, to possibly two or three hours as given by Professor Titchener in his 'Text-book in Psychology,' page 291. In this paper the author hopes to substantiate the opinion of the former on the subject by giving the results of a series of experiments carried on while in the University of Michigan in 1910.

The material used for observation was different from that hitherto used in such experiments and the method is more direct. Instead of using faint lights or liminal differences for observations we used common objects of ordinary intensity or memory images. The stimuli used were dots on paper, small parts of pictures, cutaneous stimuli of various sorts, the noise of a buzzer, etc. The subject was asked to press upon a key so long as the stimulus in question occupied the attention and to lift his finger whenever attention wandered to any other object or idea. He was later asked to enumerate the objects he observed in the interval as well as to record the time during which the attention could be held. At first the subject had difficulty in recording the moment of the wandering. He would find himself thinking of something else but would not have noticed when he wandered away from the stimulus he was supposed to observe. This source of error was probably present throughout the experiments but diminished with

practice. The results record the time our subjects could attend to a supraliminal stimulus without permitting any other object or idea to occupy the field of consciousness. The objects used for observation are given in the tables.

The experiments were performed as follows: Two rooms were used, one for the apparatus and one for the subject. In the apparatus room was a revolving drum, driven by an electric motor, upon which was placed smoked paper. An electric marker recorded upon this drum the fluctuations of attention as observed by the subject in the adjoining room. The subject pressed a button whenever he noticed a change and this marker recorded it upon the smoked paper. The time was marked off by a time-marker. The room in which the subject sat was a dark room, illuminated by artificial light. The only purpose of this was to get a tight room so as to exclude as many disturbing noises as possible. There were in this room a bell and a buzzer that could be set going by the operator at any time by pressing a button. There were also two copper electrodes placed on the subjects' arm by which the subject could be given an electrical stimulus, regulated by the operator. These last were used only as distracting stimuli to determine the effect upon the duration of attention.

The subjects for these experiments were Dr. John F. Shepard, assistant professor in psychology in the University of Michigan, Dr. Woodrow, assistant in psychology in Columbia at the time these experiments were performed, and Messrs. Harry W. Crane, R. Thane Cook, John E. Winter, and Wm. B. Fullerton, all of whom, except Mr. Cook, were graduate students taking special work in psychology.

The detailed tabulations of separate experiments contain mainly the experimental findings with Dr. Woodrow and Mr. Fullerton. The reason for choosing these two is that they served as subjects throughout the experiments while Messrs. Crane, Cook and Winter served for short periods. The variety of stimuli used with the latter subjects was also much less. As to the results obtained from the last named subjects they did not differ materially in time or content from those of Dr. Woodrow and Mr. Fullerton. At the end of the article the general results of all subjects are given.

We may first give a few tabulated results of the different experiments with the list of stimuli or ideas that came into consciousness in the intervals between observations of the stimulus in question. Relatively few are chosen but they will serve to give an idea of the way the experiments were carried on, and of the particular results.

In certain of the experiments some continuous stimulus was applied to determine what effect a distraction would have on the results. This is noted in the tables by the words after the + sign.

Tabulation of Results

Record I[1]

Exp.	Sub.	Stimulus	Sh.	Lg.	No.	Aver.	Med.	M.V.
1	*S*	Point in a picture	.2″	4.9″	11	2.7″	3.″	.98″
2	*S*	Point in a picture	.19	3.3	17	2.22	2.3	.656
3	*S*	Stone in picture	2.8	11.4	10	6.13	5.77	3.3

Record I.—Experiment 1.—Three of the disturbing elements were noises from without, twice a door slammed, and the rest were ideas concerning this experiment.

Experiment 2.—Two of the disturbing elements were noises from without, two were from the motor, twice the eyes moved to other parts of the picture, and the rest were ideas concerning the picture. The picture was a woodland scene with birds in it. The ideas were mostly concerning the birds.

Experiment 3.—The picture used in this experiment was that of an organ-grinder. One of the disturbing elements was a noise from without, one was the motor, one was a fly that lit upon the picture, and the rest were thoughts of the organ-grinder.

Record II

Exp.	Sub.	Stimulus	Sh.	Lg.	No.	Aver.	Med.	M.V.
1	*F*	Point in a picture	.2″	4.3″	31	1.23″	.8″	.69″
2	*F*	Point in a picture	.15	2.	39	.96	.8	.54
3	*F*	Point in a picture	.1	2.9	20	1.58	1.8	.76

[1] Below are given the meanings of the various abbreviations used: Exp., the number of the experiment; Sub., the subject; Sh., the shortest duration of attention and Lg., the longest; No., the number of trials in each experiment; Aver., Med., and M. V. mean, respectively, average, median, and mean variation; ″, seconds.

Record II.—Experiment 1.—As far as the subject could remember all of the disturbing elements were thoughts concerning his own experiments.

Experiment 2.—Two of the disturbing elements were noises from the other room, three were from talking outside, one was a cutaneous sensation (his finger on the key), and the rest were ideas. He had thoughts of going home, of a trip down town, of a picture of the new chemical building, of his friend and his telephone, etc.

Experiment 3.—Almost all of the distractions were disturbances from without.

RECORD III

Exp.	Sub.	Stimulus	Sh.	Lg.	No.	Aver.	Med.	M.V.
1	*W*	Point in a picture	.1″	7.7″	27	.947″	.6″	.931″
2	*W*	Point in a picture	.1	3.1	34	.98	.7	.654
3	*W*	Point in a picture	.01	2.3	22	.495	.4	.31
4	*W*	Point in a picture	.05	4.6	42	.767	.2	.76

Record III.—Experiment 1.—Two of the disturbing elements were his own breathing, two or three were noises from without, three were voluntary movements, two were flies buzzing, a number were shifts to other parts of the picture, and the rest were ideas concerning the experiment. The subject said that he had a tendency to neglect the point to examine what was going on in his mind.

Experiment 2.—The first disturbance was some one pounding, then the subject became conscious of the recording key, and the rest were due to a fluctuation from the point to the key.

Experiment 3.—Two or three of the distractions were due to organic sensations; two or three to noises; and the rest were thoughts concerning Dr. Shepard, his friend, and the picture until his foot went to sleep; then he stopped recording.

Experiment 4.—As reported by the subject, all of the disturbances were due to thoughts. Some of them were connected with the picture, but the most of them were memories of a visit that he had had that summer.

RECORD IV

Exp.	Sub.	Stimulus	Sh.	Lg.	No.	Aver.	Med.	M.V.
1	*W*	Point in a picture	2.6″	3.6″	4	3.18″	3.25″	.35″
2	*W*	Point in a picture	.6	5.3	6	2.5	2.05	1.5
3	*W*	Point in a picture	1.2	2.8	3	1.86	1.5	.65
4	*W*	Point in a picture	.15	2.6	7	1.18	.5	.84
5	*W*	Point in a picture	.6	4.	9	1.7	1.5	.7
6	*W*	Point in a picture	.2	2.9	7	1.18	.9	.6
7	*W*	Two lines in a picture	.01	5.65	12	1.83	.7	1.47
8	*W*	Point in a picture	.4	1.9	7	1.26	1.1	.51
9	*W*	Point in a picture	.01	4.3	71	.553	.3	.496
10	*W*	Point in a picture	.01	5.4	81	.57	.2	.49–

Record IV.—Experiment 1.—The following are the disturbing elements in order of their appearance: the motor, idea of[1] . . . , breathing, and a sensation in nose.

Experiment 2.—In order of their appearance the first disturbing element was an idea of a man, the second was an idea of a bird, the third was a pain in the head, the fourth the subject noticed the recording key, and the rest he forgot.

Experiment 3.—The picture used in this experiment had a religious theme. The disturbances appeared as follows: The first two were ideas of Christ, the third was the idea of a figure two, and last he forgot.

Experiment 4.—The first distracting stimulus was the idea of a tulip, the next of a spade, for the next four the attention was drawn to the recording key, and the last was the idea that "I had better cut this out."

Experiment 5.—For the first two distracting stimuli the attention was drawn to the recording key, and the rest of them were organic sensations; lump in the throat, headache, etc.

Experiment 6.—The following are the distracting elements in their order: an idea of a girl's hand, her name, an organic sensation, and the last four he began to think that this is the third, fourth, fifth, and sixth.

Experiment 7.—In this experiment the subject noticed the key three times, his breathing once, and the rest were ideas, such as "I can keep these two lines in mind," a letter H, two lines, and the rest were about a sick friend.

Experiment 8.—I might say that the picture used for record

[1] Where blank spaces are left the introspections are too personal to permit of publication.

four was an animal picture containing all sorts of animals. The disturbing elements were all ideas as follows: lion, ass, dot, girl, tough, girl, ——.

Experiment 9.—Most of the disturbances in this experiment were explorations of other parts of the picture, a number of times he attended to the key; then he became conscious of what he was doing, the rest were divided between some one talking and sensations from the wrist.

Experiment 10.—The attention was drawn by a fly crawling on the picture and flying around, by some one walking, and the rest by ideas concerning a conversation with Dr. Shepard and other things which were personal.

RECORD XXII

Exp.	Sub.	Stimulus	Sh.	Lg.	No.	Aver.	Med.	M.V.
1	*W*	Point in picture plus weak current	.01″	8.6″	38	1.98″	1.5″	1.22″
2	*W*	Same as 1 plus strong current	.1	2.9	80	.66	.3	.486
3	*W*	Same as 1 plus very strong current	.15	5.4	12	2.33	2.2	1.56
4	*W*	Same as No. 3	.01	4.5	27	1.68	1.3	1.05
5	*W*	Same as No. 3	.4	8.4	14	2.75	1.2	1.96
6	*W*	Point in picture plus very strong current	.2	8.7	26	2.55	2.1	1.44

Record XXII.—Experiment 1.—The picture used for this record was an animal picture containing many different kinds of animals. The subject noticed the current twice, contrast bands once, and the rest were ideas. He thought of a moose, a water hole, an eel, a fish, a lake at home, etc.

Experiment 2.—The attention was drawn mostly to the current, although there were many ideas.

Experiment 3.—These were all ideas concerning the animals.

Experiment 4.—The subject noticed the current twice, the recording key three times, the point shifted five or six times, and the rest were memories too personal to print.

Experiment 5.—The most of the disturbances were due to the current.

Experiment 6.—The current was noticed a number of times; this suggested a massage, which, in turn, suggested a barber shop in New York City; the subject noticed his light once and he swallowed once.

RECORD XXIII

Exp.	Sub.	Stimulus	Sh.	Lg.	No.	Aver.	Med.	M.V.
1	*W*	Point in picture	.1″	5.6″	24	2.16″	1.6″	1.27″
2	*W*	Point on white paper	.02	5.7	26	2.19	2.	1.2
3	*W*	Point in a picture	.3	8.05	21	3.06	2.1	1.84
4	*W*	4-volt light in dark room	.1	6.7	54	1.13	.4	.79
5	*W*	Image a dot	.2	8.4	8	5.34	6.	2.3

Record XXIII.—Experiment 1.—The subject at first noticed his pulse and breathing two or three times, saw some lines near to the point, and the rest were ideas. He thought of ——, of ice, of the girl working with Mr. Carpenter, of his position for next year, of a glass of beer, and lastly "What a funny line of ideas."

Experiment 2.—With the following exceptions the disturbances were all memories which are personal. He noticed his breathing twice, his recording key twice, two or three shadows, and a contrast band.

Experiment 3.—The disturbances in this experiment were all personal memories of New York City.

Experiment 4.—All of these disturbances were memories, of which the following are a few: beer, champagne, Prof. Woodworth, Barnard Psychological Laboratory, a girl that used to cry when put into the dark room, etc.

Experiment 5.—The most of the time the subject was wondering if he had the image or not. The rest, with one exception, were personal memories.

RECORD XXXIII

Exp.	Sub.	Stimulus	Sh.	Lg.	No.	Aver.	Med.	M.V.
1	*F*	Dot on white paper	1.7″	2.9″	5	2.38″	2.4″	.34″
2	*F*	Point pressed on finger	1.5	4.5	3	3.	3.	1.00
3	*F*	Point pressed on finger	1.8	6.7	5	3.98	3.8	1.02
4	*F*	Image a dot	2.	3.8	6	3.16	3.6	.53
5	*F*	Dot on white paper	3.	3.6	5	3.26	3.2	.18
6	*F*	Image a dot	1.5	4.15	8	2.78	2.7	.55

Record XXXIII.—For the next six experiments the distracting elements are given in order of their appearance.

Experiment 1.—(1) The subject thought of a lead pencil, (2) saw the shadow of his hand, (3) became aware of the key, (4) forgot, (5) saw some contrast bands.

Experiment 2.—(1) He received a sensation from his leg, (2) forgot, (3) noticed his bowels roll.

Experiment 3.—(1) The subject thought of a crooked nail, (2) he had a burning sensation on his finger, (3) he imagined a nail going through a paper, the last two he forgot.

Experiment 4.—All of the disturbances were due to the image changing shape except one, for this one he imagined a cone on legs turning around.

Experiment 5.—(1) The subject's attention shifted to another part of the picture, (2) he thought of the cone in the last experiment, (3) he thought of going to Washington, (4) he saw a contrast band, (5) this one he forgot.

Experiment 6.—(1) The image got awfully big, (2) it got large again and began to roll, (3) the dot got square, (4) he forgot this one, (5) a white ring formed around the dot, (6) an idea about the dot, (7) it turned square again, (8) this one he forgot.

A great variety of stimuli was used so as to examine into the different fields of consciousness, *e. g.*, visual stimuli, as some particular point in a picture or a dot on a blank piece of paper, etc.; auditory stimuli, as a hissing or a buzzing noise; tactual stimuli, as a pressure varying from very weak to very strong, or an electric current (a great deal of stress was put on these experiments and the results were the same as for the other experiments); imaginary stimuli, as to keep in mind (with the eyes closed) some single thing as a point that they had seen, a kernel of corn, etc. These last aroused a great deal of interest among the subjects. It was impossible to keep the stimulus still. It would get large, then small, then turn over end for end, fade out and finally disappear altogether to return in a few seconds to go through its contortions again. The disturbing elements, or the things that came in to take the place of the stimulus attended to, were as a rule ideational. Sometimes the respiration, a slight headache, a noise, a fly, or some organic sensation (especially when the subject was new at the work) would take the place of the stimulus; but as soon as they had gotten used to the work the larger number of the disturbing factors were thought processes. Many times the

disturbing elements were suggested directly by the stimulus and at other times they seemed to have no relation, *e. g.*, in one experiment a point in a picture suggested to a subject a kernel of corn, this led to a past experience when the end board of his wagon fell out and let the back part of a load of corn on to the ground, this in turn suggested certain trouble that he had had with the people of whom he got the corn, and then other instances that happened while he was working on a ranch in Colorado came to him. Then, sometimes, the subject would wander off entirely from the experiment to his studies, to the dance the night before or to a banquet that he was going to attend the coming night, etc., yet they all served to distract the attention.

Secondly, as the subject became more acquainted with the experiments, the situation and his surroundings, and became more introspective, his attention period got shorter. A new subject lets many things come into consciousness and get out again without recording them. The stimulus might be gone for some time before he realized that it had gone; but as he became more expert there were fewer mistakes. These errors, however, would not shorten the duration of attention, but would lengthen it.

Thirdly, a complex stimulus tends to lengthen the attention period, see record No. 1, Exp. No. 3. The subjects were able to wander around within or about the stimulus itself, *e. g.*, if a complex stimulus as a house was used the subject could do a great deal of thinking in and about the house as to its shape, size, color, material, cost, etc., giving the mind a chance to fluctuate and yet not get away from the stimulus. This, however, is not holding the attention to one thing, but allowing it to shift to many.

Fourthly, suggestions work very differently with different subjects as can be seen from the tabulation of the introspections. On one subject in particular, Dr. Woodrow, it seemed to have little or no effect, while with Mr. Fullerton it played quite a part, *e. g.*, if certain things were suggested to be observed in the experiment they would often appear again to the latter, while they seldom ever came to the former.

Fifthly, a distracting stimulus as a bell, a buzzer, an electric current, etc., did not interfere with the attention but aided it if anything (see the results for Mr. Fullerton with the dot and the bell and the buzzer—Table I.). They might at first hinder, but soon the subject became accustomed to them and instead of interfering they shut out a great many intermittent, disturbing stimuli as noises and made a more ideal condition for attention, thus in some cases, tending to increase its period, or else, to do away with noises and to increase the number of ideational elements.

Lastly, a very interesting thing, to the author at least, was the fact that in the introspections the disturbing elements forgotten were, almost without exception, the last ones.

The differences in opinion as they appear in the literature of attention, are due, undoubtedly, not so much to a difference in knowledge of facts, as to a difference in point of view. One is talking about the focal point of consciousness or how long *one* thing can be kept in consciousness, while the other is talking about a general consciousness or how long we may be conscious *about* a thing. It is quite a different problem to ask how long one can keep an isolated thing in consciousness, as *e. g.*, to image a mere point or to keep any single image in mind, from asking how long one can think *about* a thing. In the latter case we might be attentive in and about a thing for days in our waking hours, but this is not holding attention to a single thing but to thousands. To keep the attention on one thing for any length of time is almost an impossibility. Consciousness in its very nature (see Titchener's 'Text-book in Psychology,' pages 15 and 16) is a stream, a continual flux, and cannot be held still to a single particular, but as soon as one thing enters another follows according to the law of association. It is also very important to distinguish between attending to an object continuously, and being morally certain it has been continuously present.

This continual flux of attention might be, and properly, called the attention wave. What has been called the attention wave, heretofore, are phenomena due or related, undoubtedly, to the vaso-motor or Traube-Hering wave or to similar physio-

logical processes. The change is altogether too slow to be called the attention wave. The periodic coming and going, *e. g.*, of a faint light in a dark-room, or any minimum stimulus, is very closely related to the rhythmic changes of the body determined perhaps by a center in the medulla. It is quite a different phenomenon from keeping attention fixed upon some single supraliminal stimulus.

The average duration of attention is in the neighborhood of two seconds. This conclusion is drawn from the results of work covering sixty laboratory periods for sixty days, during which time 441 experiments were performed that gave a total of 11,816 values. From these the following general averages may be given for the different observers:

Table I

For Mr. Fullerton

For Visual Stimuli

	Aver. Aver.	Aver. Med.	Aver. M.V.
Point in picture	2.037	2.099	.640
Point in picture plus weak current	2.411	2.390	.770
Point in picture plus moderate current	2.013	1.800	.743
Point in picture plus strong current	1.580	1.550	.620
Point in picture plus a suggestion	2.130	1.958	.905
Point in picture plus buzzer or bell	2.066	1.881	.719
Dot on white paper	1.940	1.940	.623
Dot on white paper plus moderate current	2.130	1.958	.905
Dot on white paper plus strong current	1.160	1.000	.500
Dot on white paper plus a suggestion	1.775	1.500	.240
Dot on white paper plus buzzer or bell	2.330	2.126	.886
A 4-volt light	2.315	2.225	.520

For Cutaneous Stimuli

Point pressed on finger	2.480	2.450	.747
Point pressed on finger plus buzzer	1.620	1.500	.470
A moderate current on arm	1.170	.600	.780

For Image as Stimuli

Image of a single thing	2.102	1.983	.621
Image of a single thing plus suggestion	2.120	1.830	.660
Image of a single thing plus buzzer or bell	2.220	2.200	.620

Table II

For Dr. Woodrow

For Visual Stimuli

	Aver. Aver.	Aver. Med.	Aver. M.V
Point in picture	1.888	1.410	.966
Point in picture plus weak current	2.310	2.050	1.130
Point in picture plus moderate current	1.898	1.070	.989
Point in picture plus strong current	2.042	1.320	1.330
Point in picture plus a suggestion	1.540	1.230	.671
Point in picture plus buzzer or bell	1.850	1.950	.910
Dot on white paper	1.700	1.390	.900
Dot on white paper plus weak current	1.840	1.100	1.360
Dot on white paper plus moderate current	1.735	1.113	1.128
Dot on white paper plus strong current	2.020	1.350	1.320
Dot on white paper plus a suggestion	1.440	1.450	.520
Dot on white paper plus buzzer or bell	1.886	.960	1.272
Dot on white paper plus moderate current and buzzer	1.930	1.350	.830
A faint light (four volts)	1.345	.820	.710
For Auditory Stimuli			
The hum of the motor	0	1.700	1.380
The hissing of the radiator	0	1.820	.800
For Cutaneous Stimuli			
Point pressed on finger	2.417	1.825	1.372
Point pressed on finger plus buzzer	1.600	1.000	1.503
Pain (sharp point pressed on finger)	.360	.300	.760
A moderate current on arm	1.440	1.000	1.090
For Image as Stimuli			
Image of a single thing	2.620	2.216	1.380

Table III

The following are the average results for the different senses for Mr. Fullerton.

	Aver.	Aver. Med.	Aver. M.V.
For visual sensations	1.990	1.869	.672
For cutaneous sensations	1.76—	1.520	.665
For memory images	2.144	2.004	.634

Table IV

The following are the average results for the different senses for Dr. Woodrow:

	Aver.	Aver. Med.	Aver. M.V.
For visual sensations	1.816	1.326	1.002
For auditory sensations	2.070	1.760	1.090
For cutaneous sensations	1.454	1.031	1.181
For memory images	2.620	2.216	1.380

Table V

The following are the total averages for the different subjects:

	Aver.	Aver. Med.	Aver. M.V.
For Mr. Fullerton	1.9643	1.7976	.637
For Dr. Woodrow	1.9900	1.5832	1.164
For Dr. Shepard	2.5322	1.6232	.862
For Mr. Crane	1.9023	1.6621	.730
For Mr. Cook	1.7613	1.7536	.891
For Mr. Winter	1.9832	1.7301	.932

In way of summary, the duration of attention, according to these experiments, is very short; the average duration of attention is but a little over two seconds, the average median of attention is 1.69 seconds, and the average mean variation (average deviation) is less than .9 of a second; the disturbing elements were in the majority ideational, or memories; the average duration of attention is much longer with a complex stimulus than with a simple one; suggestions function as disturbing elements with some subjects while with others it does not; and a distracting stimulus as a bell or a buzzer if continuous does not ordinarily interfere with the attention.

Addendum

After this manuscript had been made ready for the press I received the results of the work of Mr. Ernest Work who had carried on a series of experiments with a view to determining the duration of attention before I began mine. As I cannot very well embody them in my own work I shall give them in this addendum. Mr. Work carried on these experiments while in the University of Michigan but gave them up and his results when he left.

Mr. Work experimented with fifteen different subjects but he, like myself, worked with some of them only a short time while he worked a whole semester with others. Two or three of his subjects had had experience in introspecting and the rest had had none.

Souvenir postal cards were used for all of these experiments. The tests were all visual. For the first set of experiments, eight subjects were tested. "The subject directed his attention

upon some particular point in the picture to the exclusion of all else." The following are the average results:

The average number of trials in each experiment	10.17
The average duration of attention in all of the experiments	2.46″
The average M.V. of attention in all of the experiments	.682″

In another set of experiments Mr. Work tried to measure the duration of attention without trying to keep the attention on any definite thing. "We started with some one point or stimulus, then let the attention go at will to whatever attracted it at the moment." Six subjects were used for these experiments. The following is a summary of his results:

The average number of trials in each experiment	11.70
The average duration of attention in all of the experiments	2.55″
The average M.V. of attention in all of the experiments	.613″

"It will be seen by the above that it seems to make very little difference in the time of attention period between the trials when the object was to get back to the same spot and the trials when the attention was allowed to go at will. . . . It would seem from this that effort in attention has little or nothing to do with the length of time that attention may be kept upon one thing."

"Another point of interest got from a number of subjects . . . is that the change from one center of attention to another is not abrupt and all at once, but gradual. The one period fades out—shades off—into the next."

The next tests were to determine the effect of distracting stimuli on attention. Three subjects were used for these experiments. The subjects were first tested without, then with a current with the following results:

The average number of trials in each experiment (without current)	9.72
The average duration of attention in all of the experiments	2.83″
The average M.V. of attention in all of the experiments	.564″

Experiments with weak current:

The average number of trials in each experiment	10.75
The average duration of attention in all of the experiments	3.37″
The average M.V. of attention in all of the experiments	.517″

Experiments with strong current:

The average number of trials in each experiment	15.97
The average duration of attention in all of the experiments	2.10″
The average M.V. of attention in all of the experiments	.856″

"It will be seen from these experiments that a slight stimulus aids attention to an object; *i. e.*, increases the period of attention, but when a certain intensity is reached the new stimulus ceases to aid and detracts from the attention time. Individuals, however, are not all the same in this, nor is the same person the same on different days and under different conditions. It aids sometimes and detracts at others. My first subject always did better without the stimulus. All the others, however, did better with a slight stimulus."

These results are of interest to the writer as they are so similar to his own. I might say that neither of us knew of the other's work. I knew nothing of Mr. Work's methods, the points that he was investigating, or anything, other than that he had been doing work along this line. So these results were worked out entirely independently of one another with a surprising similarity in the outcome.

M. L. B.

THE EFFECT OF SIZE OF ADVERTISEMENTS AND FREQUENCY OF THEIR PRESENTATION[1]

BY EDWARD K. STRONG, JR.

Columbia University

The problem, the solution of which we hoped to reach through this experiment, is as follows: Which should preferably be run, (1) one 1-page advertisement in four months, or (2) two ½-page advertisements two months apart, or (3) four ¼-page advertisements, one each month? In other words, what is the truth with regard to the statement that "small space in many magazines is better than large space in few magazines."

The practical applications of this study have already been presented to advertising men[2] and we shall not concern ourselves with them now. But there are two points of interest to the psychologist which we shall consider here. These two points are: (1) how does an increase in the size of an advertisement affect the permanency of impression made upon the reader? and (2) how does continued repetition of a firm's advertisement affect this permanency of impression? Stating the problem in concrete language we have (1) what is the efficiency of a ¼-page advertisement as compared with a 1-page advertisement and (2) what is the efficiency measured in the permanency of impression of a firm who advertises once in four months and a firm who advertises four times in the four months? These two problems are purely psychological in nature. They are problems that the psychologist ought to be able to answer. But they are problems which the psy-

[1] This report presents the psychological aspects of a study made for the Association of National Advertising Managers whose research fellowship I hold.

I wish to express here my indebtedness to Prof. R. S. Woodworth and Dr. H. L. Hollingworth and to their assistants, Mr. G. F. Willianson and Miss Mabel Barrett, for the help they have afforded me in securing subjects for this experiment.

[2] 'Size and Frequency in Advertising,' Association of National Advertising Managers, Research Bulletin No. 4, April 1913.

chologist has not been able to answer except on the basis of theories which he would not be at all sure would apply in this particular case.

The Experiment

The experiment was as follows: After a great deal of care the advertisements of 144 different firms were selected for use. One third of these firms were using 1-page advertisements, one third were using ½-page advertisements, and the remaining third were using ¼-page advertisements. Four advertisements were used from 12 firms from each of the above three groups, two advertisements were used from 12 other firms from each of the three groups, and one advertisement was used from the remaining 24 firms in the three groups. In this way 288 different advertisements were employed in all.

These 288 advertisements were divided into four sets corresponding to four monthly issues of a standard magazine. In each of these four sets there was one advertisement from each of the 36 firms of which 4 advertisements were being used. In other words, these 36 firms advertised in each one of the four dummy magazines. The firms of which 2 advertisements were used from each, were represented in alternate sets. In other words these firms advertised every other month in the dummy magazines. And in the same way the firms of which only one advertisement was used advertised once in the four issues of the dummy magazines.

We have then, to repeat, the following situation:—

12 firms using full-pages and advertising 4 times.
12 firms using full-pages and advertising 2 times.
24 firms using full-pages and advertising 1 time.
12 firms using half-pages and advertising 4 times.
12 firms using half-pages and advertising 2 times.
24 firms using half-pages and advertising 1 time.
12 firms using quarter-pages and advertising 4 times.
12 firms using quarter-pages and advertising 2 times.
24 firms using quarter-pages and advertising 1 time.

Two ½-page advertisements were pasted on a sheet the size of a full-page and four ¼-page advertisements were treated in the same way. The 1-page advertisements were similarly treated. Position of the half and quarter page advertisements

on the sheets was determined by chance but care was taken that advertisements from the same firms would not appear twice together on any page. In presenting the advertisements to the persons tested, the order of the sheets was constantly changed by shuffling them. In this way no advertisement had any advantage over any other advertisement through the position of that advertisement in the set.

Two different methods of presenting the advertisements were followed. With half of the subjects the sheets were shown at a uniform rate of one per second. With the other half the sheets were given to the subjects and they were instructed to look them through at their leisure. It was suggested that they look them through in the same way that they would turn the pages of an advertising section of any magazine looking at what interested them and ignoring the rest. In each case they were timed by a stop-watch. The first method was employed to satisfy the experimenter, the second to satisfy advertising men. But as both methods give us proportionately the same results, it is not necessary to discuss their relative merits.

Sixteen men and fifteen women were tested by the first method and nine men and fourteen women by the second method. Due to sickness, dropping college, etc., only ten men and eleven women of the first group and six men and twelve women in the second group completed the work. Although but 39 persons in all were tested, the uniformity of the results and the low probable errors are evidence that the results of the experiment are very reliable.

The four sets of advertisements were shown to the subjects a month apart. One month later they were tested as to their remembrance of what had been shown them. In this test they were shown the last advertisement shown them from each firm together with an equal number of wrong advertisements. They were instructed to pick out all the advertisements which they had seen previously in the test. If they were sure any advertisement had been seen before they were instructed to pick it out. Moreover, if they were not sure that the advertisement before them was the one they had seen, but they were sure it was the same *firm*, that was sufficient.

A word might be said as to the real value of a recognition test, as used here, for determining the permanent impression of a series of advertisements. Psychologically the situation which most advertising is aimed to meet is first and primarily, the development of a very strong associative bond between *a need for a commodity* and *a trade-name*, as *need of soap* and *Ivory*, and second, the development of a very favorable attitude toward that trade-name. There is no desire to develop associative-bonds between a certain magazine and the advertisements displayed in it. Inherently then a recall test is of little or no value. In fact, the writer agrees with some advertising writers when they state that an advertisement remembered for its own sake (as it must be in a recall test) is very much of a failure. For the attention of the reader has been directed not to connecting a need with a remedy but to praise of an advertisement as an advertisement. The recognition test, on the other hand, tests what the reader paid attention to originally when he looked through the magazine. Nearly all will agree that advertisements that were *not* paid attention to in the magazine will not be recognized later,—except on mistaken grounds. And as my work has already shown that under ordinary conditions very few advertisements are selected which have not been shown before, we are not bothered in this test particularly by such mistaken recognitions. The only criticism of the recognition test lies in the fact that a reader *might* pay equal attention to three advertisements, for example, and yet not recognize one of them later. This seems impossible to the writer but after all we do not know the facts. The recognition test then determines which advertisements were noticed originally and which were not. And when degrees of certainty of the recognitions are asked for then we obtain some light also as to the strength of the impression.[1]

(The Treffer- und Zeitmethode might be employed in this connection in this way. After the advertisements have been displayed, present a list of the commodities that have been advertised and determine the trade-name given and the time

[1] Further consideration of this point is given in 'Psychological Methods as Applied to Advertising,' *Jour. of Educ. Psychol.*, *4*, 1913, 393–404.

of response. But the weakness of such a procedure lies in the difficulty of determining before the experiment commences the strength of the associations then in existence between commodity and trade-name and also the increase of associative strength that takes place between exposure and the test from other sources than those included in the test itself. This second difficulty is probably insuperable when an experiment continues for four months, for the advertising seen on every hand can not be excluded from the subjects.)

Before turning to the results of this experiment it is well to consider some of the precautions that were observed in the work.

1. Only advertisements from the twelve issues of the 1911 *Everybody's* magazine were used. As the advertisements were old ones, they were not likely to be seen outside of the test.

2. On the basis of the experimenter's and his wife's judgments all advertisements were excluded of firms which the average person thinks of as being always present in the standard magazine, such as Ivory Soap. All clues were thus eliminated as to what advertisements ought to have been in the four sets.

3. But one commodity of any firm was used in the test. Hence if the firm name was remembered full credit would be given to the advertisement which displayed it.

4. No two firms using full-page advertisements advertised the same commodity. The same was true of the firms using half-page and quarter-page advertisements. There were a few instances where the same commodity but of different firms was advertised in different sized advertisements. This was necessary because of a lack of the requisite advertisements. The error arising from these few cases, if there was any, would be too small to affect the results in an appreciable manner.

5. The individuals tested were allowed to assign a grade of 100 per cent., 75 per cent., or 25 per cent. validity to the recognitions of each advertisement which they selected as having been shown them before. In this way those advertisements which had made the most impression on each individual could be considered separately from those advertisements which were remembered but had made slight impression.

6. Pure guessing in the tests on the part of the subjects was eliminated through an elaborate system of grading the data.[1] For example, a person who picked out an equal number of right and wrong advertisements was not considered since chance could account for such results. In a similar way the more accurate the work of an individual the more it counted toward the final results.

The Results of the Experiment

1. *The Total Per Cent. of Firms Remembered.*—Table I. presents the results on this point. The per cents. given in this table seem very low. But when it is remembered that the advertisements were shown in four sets separated by an interval of a month and that the test followed a month later one should not be surprised at the results. In fact they are higher, especially in the case of the second method, than the experimenter expected.

Table I

Showing the Per Cent. of Firms Remembered per Reader

	Method of Presentation	Per Cent. of Firms Remembered Per Reader
1st method.	Pages shown at rate of 1 per sec.	2.1 per cent. (P. E. *0.4*)
2d method.	Pages shown at leisure of reader.	6.3 per cent. (P. E. *0.8*)

The individuals who were allowed to look at the advertisements at their leisure remembered 3 times as many as the individuals who were allowed but one second per page. The former group average 219 seconds per set while the latter had about 63 seconds. The former group spent 3½ times as much time in looking at the advertisements and remembered 3 times as much. This result agrees very well with Ebbinghaus's results where he found a direct proportion between the number of repetitions (or time spent on a series) and the saving in learning the series 24 hours later.[2]

There is a correlation of +.65 between the time spent by the subjects of the second method in looking at the advertise-

[1] For a general description of the method employed here, see the writer's article, 'The Effect of Length of Series upon Recognition Memory,' Psych. Rev., November, 1912, 19, 447–462.

[2] H. Ebbinghaus, 'Das Gedachtnis,' 1885, Chap. VI.

ments and the per cent. recognized. This correlation would be raised to +.80 if the records of one individual were omitted. In a much more difficult test made on these same subjects immediately after the one described here a correlation of +.82 is obtained between time spent in looking at the advertisements and the per cent. recognized. And if the same individual is again omitted the correlation is raised to +.88. It seems very surprising to the writer that there should be such a high correlation between these two factors, since there is a very great variation between the records of subjects when the advertisements are presented at the rate of one page per second.

2. *The Per Cent. of Firms Remembered for Each of the Nine Combinations of Space and Frequency.*—Table II. gives the percentage of firms remembered per reader for each of the nine combinations of space and frequency. In order to get clearly before us the meaning of these figures we shall consider first the factor of *size of space* and then the factor of *frequency* and after that return to this table for a general view taking into consideration both of the factors.

Table II

Showing the Per Cent. of Firms Remembered per Reader for Each of the Nine Combinations of Space and Frequency[1]

1. *When the Pages Were Shown at the Rate of One per Second*

Frequency	Size of Advertisements			Probable Errors of Averages		
	¼-page	½-page	1-page	¼-page	½-page	1-page
Once	1.7	2.3	3.6	*0.2*	*0.3*	*0.4*
Twice	2.2	3.1	4.6	*0.3*	*0.6*	*0.7*
4 times	3.0	3.9	6.0	*0.6*	*0.7*	*0.8*

[1] The figures in this table and those of Table I. do not correspond exactly. In Table I. chance selections which were correct were cancelled in the total score by corresponding chance selections which were incorrect. In Table II. this could not be done in such a way that the writer could feel sure the process had not influenced the results he was endeavoring to obtain. Table II. contains then results certainly to be attributed to genuine memory plus a certain amount undoubtedly due to chance. The latter factor is larger in the results from the first method of presenting the advertisements than from the second method. The writer does not feel that the presence of these chance results can affect the general results obtained in the experiment, while their elimination by the only method known to him might have done so.

2. *When the Pages Were Viewed at the Reader's Leisure*

Frequency	Size of Advertisements			Probable Errors of Averages		
	¼-page	½-page	1-page	¼-page	½-page	1-page
Once.	3.8	5.9	8.7	*0.5*	*0.7*	*0.9*
Twice.	4.4	7.0	10.5	*0.8*	*1.0*	*1.3*
4 times.	6.5	8.2	13.0	*1.0*	*0.8*	*1.4*

3. *Value of Space.*—If now we take the per cents. in the two parts of Table II., and express them in terms of ratios, letting the value of the ¼-page advertisement each time represent 1.00, we have the proportions shown in Table III.

Table III

Showing the Value of ¼-page, ½-page, and 1-page Space in Terms of Ratios of ¼-page Space

Frequency	Rate of Exposure	¼-page	½-page	1-page
Firms advertising once.	1 per second.	1.00	1.33	2.12
	Leisure of reader. . .	1.00	1.55	2.29
Firms advertising twice.	1 per second.	1.00	1.41	2.09
	Leisure of reader. . .	1.00	1.59	2.39
Firms advertising 4 times.	1 per second.	1.00	1.30	2.00
	Leisure of reader. . .	1.00	1.26	2.00
Average of all 6 ratios.	. . . .	1.00	1.41	2.15

These figures indicate that a ½-page space is not twice as efficient as a ¼-page space but only 41 per cent. more efficient. They also indicate that a 1-page space is only twice as efficient as a ¼-page space although occupying four times the area. Moreover, this relationship between the three sized advertisements considered here holds equally well whether a firm runs one advertisement, two advertisements, or four advertisements. In each case the ratios of efficiency remain approximately 1.00 : 1.41 : 2.15.

In this connection let us consider some other evidence bearing on this point.

(*a*) In our experiment[1] with *Everybody's* magazine upon

[1] The four experiments given here were by-products to various experiments and the particular facts stated here have not been published before. The ratios given here can only be considered as approximations to the true relationship due to the presence of varying conflicting factors.

36 women who did not know that they were going to be tested (eliminating full-page advertisements in preferred position) the ratios of efficiency for ¼-page, ½-page, and 1-page advertisements were: 1.00 : 1.11 : 1.13.

(*b*) In a similar experiment with the *National Geographic Magazine* with 56 persons who did not know they were going to be tested the ratios were: 1.00 : 2.39 : 3.65.

(*c*) And upon 60 persons who knew they were going to be tested the ratios were: 1.00 : 1.53 : 2.34.

(*d*) In another experiment where 2 ½-page advertisements together were compared with their efficiency when appearing one at a time on a page, and similarly with the ¼-page advertisements. The ratios obtained in this case were: 1.00: 1.60 : 2.51.

The averages of these four relationships are: 1.00 : 1.66 : 2.41.

Besides these experimental results, I quote this one from practice.

(*e*) The ratios between inquiries per advertisement from 10 ¼-page advertisements, 19 ½-page advertisements, and 22 1-page advertisements, as given by Shryer[1] are: 1.00 : 1.61 : 2.26.

All these ratios show that a 1-page advertisement is not twice as efficient as a ½-page advertisement nor four times so efficient as a ¼-page advertisement and (with one exception) that a ½-page advertisement is not twice so efficient as a ¼-page advertisement. On the other hand they all approximate much more closely to the ratio: 1.00 : 1.41 : 2.00 than to the ratio 1.00 : 2.00 : 4.00. In other words the efficiency of space increases approximately as the square root of the area and not directly as the area (1.00 : 1.41 : 2.00 is the same ratio as that of $\sqrt{1} : \sqrt{2} : \sqrt{4}$).

Scott's[2] deduction from experiments carried out by him give just exactly the opposite result. He states 'that the full-page advertisement was more than twice as effective as a half-page advertisement and a half-page was more than twice as effective as a quarter-page.' His results are based on experiments in

[1] W. A. Shryer, 'Analytical Advertising,' 1912, p. 190.

[2] W. D. Scott, 'The Psychology of Advertising,' 1908, p. 172.

which the reader afterwards recalled what he could of the advertisements he had seen. The writer has found this method very inadequate, as many persons, even when knowing they are going to be tested, can carefully look through a magazine's advertising sections and yet recall but one or two advertisements. Moreover, those that they do recall are generally the advertisements that are always to be found in the magazine, such as Ivory soap. Scott did not even guard against this tendency for he tells us that the Ivory soap advertisement was most often mentioned, the In-er-Seal advertisement was next and Pears' soap was third. In a recent letter to the writer Scott admits that "the better data from the full-page advertisements than from the smaller ones and more than would be expected because of the increase in size . . . may have been the result of previous education and not of the independent observation at the time of the experiment."

Scott also employed a recognition test. The results from that test indicate in one case that a full-page advertisement is not quite twice so effective as a half-page advertisement. Beyond this one contradiction to the results obtained by the recall-method his two experiments agree as to the general conclusion stated above. A number of precautions must be observed in such experiments, *e. g.*, (1) the elimination of preferred position, which only benefits full-page advertisements; (2) the elimination of well-known firms, which benefits full-page advertisements much more than smaller space advertisements; and (3) the weighing of the accuracy of the recognitions, else careless and cocksure individuals will influence the totals much more than careful and conservative individuals. Scott gave some attention to the factors of preferred position and of well-known firms but no attention to the weighing of the accuracy of recognitions. In the writer's opinion these factors, not completely guarded against by Scott, account for the difference between his results and those of this report.

We must conclude then that the value of space increases approximately as the square root of the increase in area and not directly with the increase in area. A ¼-page space is worth $^7/_{10}$ of a ½-page space and ½ of a full-page.

4. *Value of Frequency of Repetition When There is a Considerable Interval of Time between Each Presentation.*—Taking the per cents in Table II. and expressing them in terms of ratios, this time letting the value of one presentation represent 1.00, we have the proportions shown in Table IV.

TABLE IV

SHOWING THE VALUE OF ONE PRESENTATION, TWO PRESENTATIONS, AND FOUR PRESENTATIONS IN TERMS OF ONE PRESENTATION. (Based on Table II.)

Size of Ad.	Rate of Exposure	Number of Presentations		
		One	Two	Four
¼-page	1 page per second	1.00	1.29	1.76
	Leisure of reader	1.00	1.16	1.71
½-page	1 page per second	1.00	1.35	1.70
	Leisure of reader	1.00	1.19	1.39
1-page	1 page per second	1.00	1.28	1.67
	Leisure of reader	1.00	1.21	1.49
Average of the 6 ratios		1.00	1.25	1.62

It is evident from these ratios that two presentations add only one fourth more to what one presentation has accomplished and that four presentations add only two thirds more to what one presentation has accomplished. Off-hand one might have expected ratios of 1.00 : 2.00 : 4.00. That is, that two presentations would be twice as effective as one, and four presentations four times as effective as one. But we do not get this relationship at all. It is also evident that there is no appreciable difference between ¼-page, ½-page, or 1-page space as far as the relative increase of permanent impression is concerned due to repetition. In other words repetition increases the value of one presentation of the three sized advertisements in the same way, by adding one fourth more with a second presentation and two thirds more with three more presentations.

If the two methods of presenting the advertisements to the subjects are considered separately we have the following ratios as to the value of one, two, and four presentations:

Ads shown at rate of 1 page per second, 1.00 : 1.31 : 1.71.
Ads shown at the leisure of the reader, 1.00 : 1.19 : 1.53.

Apparently from these figures, the individual who looks at advertisements at his leisure pays relatively more attention to the first presentation of a firm's advertisements than to the succeeding presentations than does the individual who has but one second to peruse a page. But for the purposes of this experiment an average of the two sets of ratios is the safest measure of the relationship.

Conclusion.—In this case repeated presentations do not have proportionate efficiency. One, two, and four presentations do not have ratios of 1.00 : 2.00 : 4.00 but rather ratios of efficiency of 1.00 : 1.25 : 1.62. (Curiously these ratios are approximately equal to the cube roots of the number of presentations, *i. e.*, $\sqrt{1} : \sqrt[3]{2} : \sqrt[3]{4}$ equals 1.00 : 1.26 : 1.59.)[1]

5. *Value of Space and Frequency Considered Together.*—We have seen already that the value of space does not increase proportionately with the increase in area but that when

¼-page space equals an efficiency of 1.00, then
½-page space equals an efficiency of 1.41, and
1-page space equals an efficiency of 2.15.

And we have seen that the values of successive repetitions a month apart of a firm's advertisements do not increase proportionately with the increase in presentations but that when

1 presentation equals an efficiency of 1.00, then
2 presentations equal an efficiency of 1.25, and
4 presentations equal an efficiency of 1.62.

Table V

Showing the Value of the Nine Combinations of ¼-page, ½-page, and 1-page Space Presented Once, Twice, and Four Times.

Each value is stated in terms of the value of a ¼-page advertisement shown once. (Ratios obtained from averaging the two parts of Table II.)

Number of Presentations	Size of Advertisement		
	¼-page	½-page	1-page
1	1.00	1.45	2.20
2	1.22	1.83	2.73
4	1.73	2.22	3.47

[1] The ratios obtained here apply to *intervals of one month* between presentations. From data now being accumulated we find that shorter intervals, as one week, give ratios indicating a greater effect from two or four presentations than shown here.

Now combining the two factors we get the ratios shown in Table V. Here efficiency of each combination of size and frequency is stated in terms of the efficiency of a ¼-page advertisement shown once.

This table shows us very clearly that a ½-page advertisement shown once in four months is superior in the permanency of its effect to 2 ¼-page advertisements shown two months apart, for the relative values of the two are:

½ page shown once in 4 months equals 1.45
¼ page shown twice in 4 months equals 1.22

It shows us that a 1-page advertisement shown once in four months is superior in the permanency of its effect to 2 ½-page advertisements shown two months apart, or to 4 ¼-page advertisements shown once a month, for the relative values of the three are:

1 page shown once in 4 months equals 2.20
½ page shown twice in 4 months equals 1.83
¼ page shown 4 times in 4 months equals 1.73

And the table shows us that 2 1-page advertisements shown in 4 months are superior to 4 ½-page advertisements shown during the same interval, for the relative values of the two are:

1 page shown twice in 4 months equals 2.73
½ page shown 4 times in 4 months equals 2.22

It is very evident then that for the same total amount of space used during four months one obtains a greater permanency of impression by using in the same magazine large space and less often than by using small space and more frequently. It is very easy to see that this must be the case in this particular situation for permanency of impression increases approximately as the square-root of the space used but only as the cube-root of the number of presentations. Hence, to repeat, the same amount of space used in large advertisements seldom repeated must be more effective for permanent impression than when used in small advertisements more frequently repeated.

6. *Value of Space and Frequency Considered together when the Frequency Consists of Presentations Occurring within a Few Minutes of Each Other.*—The writer knows of no other experimental work where a study has been made of the value of

successive presentations in which the presentations were separated by a considerable interval of time. But there have been a number of studies where the effect of successive presentations has been studied in which the presentations occurred within a few minutes of each other. A word might be said here as to the effectiveness of such repetitions (1) as it has been found in advertising and (2) as found in other phases of mental response.

Based partly upon experiment and partly upon theory, the writer prophesied[1] that two presentations of a firm's ½-page advertisements run in the same magazine would have greater efficiency than a full-page advertisement. The results from one advertising firm, who tried the stunt, show a ratio of efficiency of 116 to 100 in favor of the 2 ½-page advertisements.

Münsterberg,[2] reports an experiment in which were determined the effectiveness of a full-page advertisement appearing once, a ½-page advertisement appearing twice, a ¼-page advertisement appearing four times, a ⅛-page advertisement appearing eight times, and a 1/12-page advertisement appearing twelve times;—all appearing in a set of 60 pages of advertising. His values based upon immediate impression are:

A 1 page advertisement shown once had a memory-value per reader of 0.33
½ page advertisement shown twice had a memory-value per reader of 0.30
¼ page advertisement shown 4 times had a memory-value per reader of 0.49
⅛ page advertisement shown 8 times had a memory-value per reader of 0.44
1/12 page advertisement shown 12 times had a memory-value per reader of 0.47

In regard to the assigned value of the 1-page advertisement shown once Münsterberg states that he was "inclined to believe that the ascent of the curve of the memory-value from the full-page to the fourth-page or eighth-page would have been still more continuous, if the whole-page advertisements had not naturally been such as are best known to the American reader. The whole-page announcement, therefore, had a certain natural advantage." This use of full-page advertisements of well-known firms undoubtedly raised the obtained value above its true value, just as we have found was the case with Scott's experiments.

[1] 'On the Amount of White Space Valuable for Attention or Isolation Value,' Association of National Advertising Managers, Research Bulletin, No. 1, October 16, 1912.
[2] H. Münsterberg, 'Psychology and Industrial Efficiency,' 1913, Chap. XX.

Smith's[1] experiment is the only other experimental study known to the writer that is directly applicable here in that the subjects were not expected to memorize the material before them but were requested simply to look at it. In that study the students read through different series of 10 nonsense syllables. They 'were directed not to try and learn as much as possible but simply to repeat, with all possible regularity what was presented to them.' The results were as follows:

After one reading of the series 22 per cent. could be reproduced.
After 3 readings of the series 25 per cent. could be reproduced.
After 6 readings of the series 28 per cent. could be reproduced.
After 9 readings of the series 34 per cent. could be reproduced.
After 12 readings of the series 39 per cent. could be reproduced.

Smith concludes that 'the first repetition is undoubtedly the best, *i. e.*, more is learned by it than by any other repetition, or in fact, by all the other (11) repetitions put together.'

In studies where the student was expected to try and learn as much as possible of a series of numbers, the results agree on the whole with the above. Thus, for example, Hawkins[2] found (averaging all his results) that after

1 reading 47 per cent. was recalled—in terms of a ratio 1.00
2 readings 42 per cent. were recalled—in terms of a ratio .90
3 readings 59 per cent. were recalled—in terms of a ratio 1.25

and Smedley[3] found that after

1 reading 47 per cent. was recalled—in terms of a ratio 1.00
2 readings 55 per cent. were recalled—in terms of a ratio 1.17
3 readings 59 per cent. were recalled—in terms of a ratio 1.25

Other experimenters, such as Pohlmann[4] and Henderson,[5] agree as to the same general relationship. But the latter finds in his study of prose passages that the longer study 'seems more effective in increasing the percentage of words and ideas

[1] W. G. Smith, 'The Place of Repetition in Memory,' Psychol. Rev., 3, 1896, pp. 21–31.

[2] C. J. Hawkins, 'Experiments on Memory Type,' Psychol. Rev., 4, 1897, 289–294.

[3] F. W. Smedley, 'Child Study in Chicago,' U. S. Bureau of Education, 1902, I., 1134.

[4] A Pohlmann, 'Experimentelle Beitrage zur Lehre vom Gedachtnis,' Berlin, 1906.

[5] E. N. Henderson, 'A Study of Memory for Connected Trains of Thought,' Psychol. Rev., Monog. Sup., No. 23, 1903.

retained' after some time. But he does not get anything like a direct ratio between the number of repetitions and the amount retained.

Ebbinghaus,[1] however, found in learning series of 16-syllable series that there was a direct relationship between the number of repetitions employed today and the number gained in relearning tomorrow. That is, if the series was repeated

	A gain Per Repetition of
0 times, it required 1,270 seconds to learn it 24 hrs. later.	
8 times, it required 1,167 seconds to learn it 24 hrs. later.	12.9 secs.
16 times, it required 1,078 seconds to learn it 24 hrs. later.	12.0 secs.
24 times, it required 975 seconds to learn it 24 hrs. later.	12.3 secs.
32 times, it required 863 seconds to learn it 24 hrs. later.	12.7 secs.
64 times, it required 454 seconds to learn it 24 hrs. later.	12.8 secs.

But if the series was relearned not as exactly studied the first day but as rearranged in this way: 1st syllable, 3d syllable, 5th syllable, . . . 15th syllable, 2d syllable, 4th syllable, . . . 16th syllable, then

	A gain Per Repetition of
0 repetitions required 1,270 seconds to learn it 24 hrs. later.	
16 repetitions required 1,170 seconds to learn it 24 hrs. later.	6¼ secs.
64 repetitions required 1,109 seconds to learn it 24 hrs. later.	2½ secs.

"Quadrupling the repetitions resulted here in increasing the saving only a little more than half as much again" (100 seconds as compared with 160 seconds). The results obtained in this last case are very similar in their general relationship to those obtained in our own results. The ratio of presentation is 1.00 : 4.00 while the efficiency is but 1.00 : 1.61. In our experiments when the ratio of presentations was 1.00 : 4.00 the efficiency was but 1.00 : 1.62.

The situation, as far as can be determined from experimental work now in existence to my knowledge is this. The effect of repeated presentations varies enormously under varying conditions. What the laws are we do not know. But it seems possible that the more difficult the situation the less proportionate are the results of repetition. For when the presentations are a month apart a single presentation of a 1-page advertisement is more efficient than four presentations of ¼-page advertisements, but when the presentations are separated by only

[1] H. Ebbinghaus, *op. cit.*

a few minutes then the reverse is the case. In other words, it seems possible that there is an optimum length of interval between successive presentations. When that interval is lengthened the effect from each presentation is weakened. But what that optimum interval is we do not know.[1]

Conclusion

1. An approximately direct relationship was found between length of exposure of advertisements and the number that could be correctly recognized on an average of 2½ months later.

2. The value of space in advertising as affecting permanent impressions increases approximately as the square-root of the increase in area and not directly with the increase in area.

3. The effectiveness of successive presentations of advertisements depends seemingly upon the interval of time between the presentations.

4. When the interval of time between successive presentations is very short, space used in advertising is most effective when broken up into small advertisements and presented in succession (according to Münsterberg).

5. When the interval of time between successive presentations is very long (a month), space used in advertising is more effective when used in a large advertisement than if presented in small advertisements and in succession.

Note.—Because of a number of practical considerations that must be taken into account the above conclusions do not apply equally to all phases of advertising. Direct application, then, can only be made when these practical considerations are kept in mind.

[1] More recent work of mine shows clearly that intervals of a few minutes or of a week are superior to that of a month. In the light of all the work on this subject, it seems probable that an interval of one day is such an optimum. See "Two Factors in Economical Learning," to appear shortly in the *Journal of Philosophy Psychology, and Scientific Methods.*

CONCERNING INDIVIDUAL DIFFERENCES IN REACTION TIMES

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The fact of marked individual differences in reaction times and in types of reaction has often been noted. Less frequently has the question been raised as to the persistence of these differences after long-continued practice. Still less frequently if ever has the correlation between the times of simple and compound reactions been especially treated.

On the persistence of differences after practice, Alechsieff[1] says, "Individual differences in transit observations have been especially referred in part to practice and in part to the employment of different modes of reaction . . . . If the observers are thoroughly practised, personal differences become vanishingly small." Similarly, Whipple[2] says, "And now most important is this: when practice is attained, as indicated by a mean variation less than 10 per cent. of the average or median value, the results judging from my experience in the laboratory at least, are practically identical for all observers. In other words, our constant individual differences have disappeared since they themselves depend upon the degree of practice and upon variation in the direction of attention." These statements do not agree with the writers' experience with reaction time measurements. Individual differences both in simple and compound reactions persist after very long periods of practice and rest on more fundamental differences than those

[1] Alechsieff, 'Reactionszeiten bei Durchgangsbeobachtungen,' Philosophische Studien, 16, 1900, pp. 26–27.

[2] Whipple, G. M., 'Reaction-Times as a Test of Mental Ability,' *Am. Jour. of Psychol.*, Vol. XV., 1904, pp. 489–498.

of practice and direction of attention. There appears also to be a surprising lack of correlation between the times for simple and compound reactions.

In the study[1] of the time of perception as a measure of differences in sensations by one of the writers, long series of experiments on differences in color and pitch were made on two subjects, *H* and *W*. The average times for discriminative or choice reactions to red and yellow (440 cases) were as follows:

H	*W*
av. 216.9σ, m.v. 17.8σ	av. 256.5σ, m.v. 22.1σ

Before taking these series *H* had made approximately 2,000 simple reactions to sound and light stimuli and 3,000 discriminative or choice reactions to colors for the purpose of eliminating practice effects. *W* had made similarly a considerable but unknown number of simple reactions and approximately 1,500 compound reactions. From the time that the published experiments with the red and yellow were begun no appreciable differences in the times due to practice were discoverable. Similar differences between the times for the two subjects were shown in the experiments on pitch. The results for the differences of 16 vibrations (320 cases) were as follows:

H	*W*
av. 290.2σ, m.v. 26.6σ	av. 344.3σ, m.v. 40.9σ

After the completion of the study from which the above results are drawn *H* and *W* began certain studies of reaction times, only a portion of which was ever completed. Before these were begun *W* had made approximately 5,000 simple reactions (to visual stimuli) and 5,000 compound reactions; *H* approximately 4,000 simple reactions (visual and auditory) and 14,000 compound reactions. So far as it is possible ever to be sure in such matters practice effects had been entirely eliminated. The high degree of practice which both subjects had attained seemed to furnish an invaluable opportunity for a study of reaction times in general. The first experiments consisted of 1,000 simple muscular reactions to auditory stimuli, the Zimmerman sound hammer being used as stimulator.

[1] Henmon, 'The Time of Perception as a Measure of Differences in Sensations,' *Archives of Philosophy, Psychology and Scientific Methods*, No. 8, 1906.

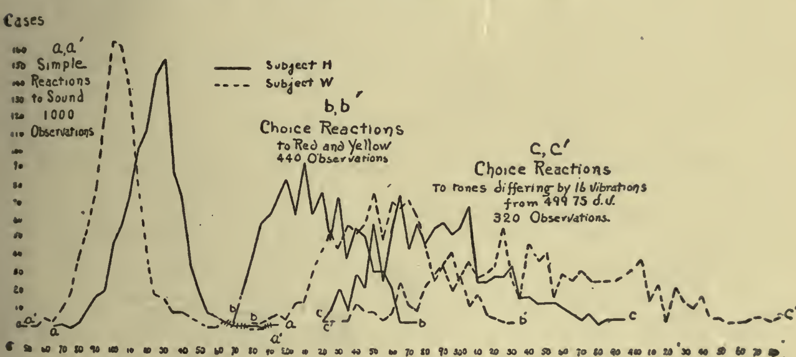
Series of 100 reactions were taken each day. The results were as follows:

H	*W*
av. 125.4σ, m.v. 10.5σ.	av. 105.3σ, m.v. 10.5σ.

A contemporary research carried out independently in the same laboratory,[1] afforded data on these same individuals concerning the simple reaction times to visual stimuli of varying sizes. The averages of all reactions taken with each subject were as follows:

H
500 reactions, av. 184.6σ, m.v. 9.9σ
W
2,000 reactions, av. 173.6σ m.v. 8.6σ

The distribution of the observations in the preceding results with the exception of those for simple visual reactions, is shown graphically in the accompanying chart.



To render the curves more easily comparable, the scale in plotting the choice reactions is altered to make it correspond to that of the simple reactions.

In the two subjects, the simple reaction times differ with the auditory stimuli by 20σ, with the visual stimuli by nearly 11σ. Of the fact of these individual differences, preserved long after practice could essentially change them, there can be no dispute. This result is distinctly contrary to the statements of both Alechsieff and Whipple. It raises the question of whether the

[1] Froeberg, 'The Relation between the Magnitude of the Stimulus and the Time of Reaction,' *Archives of Psychol.*, No. 8, 1907.

subjects of either of these writers had had sufficient practice to permit of any generalizations concerning the properties of the simple reaction in advanced stages of practise. Neither adduces definite experimental evidence to this effect. The differences here found, are then, due neither to differences in practise, nor to differences in the direction of the attention (sensory or motor), since, so far as it is ever possible to control this factor subjectively, motor reaction alone was employed. The differences represent more fundamental differences in the speed of neural conduction—more probably a difference in the facility of synaptic coördination (or some other interneuronal process) than of transmission along nerve trunks.

Distinct individual differences also exist in the discriminative or choice reactions, but in the opposite direction from those of the simple reactions. Although reacting more quickly to simple light and sound stimuli, *W* reacts much more slowly where discrimination and choice between two stimuli are involved. The differences in times for the two subjects are 40σ for colors and 54σ for pitch. It should be noted also that false reactions in the discriminative or choice reactions were throughout nearly twice as frequent in *W* as in *H*. So far as it was possible to discover (we are now unable to give the exact figures), there was no essential difference in the sensory discriminativeness at the threshold of color or pitch. The significant feature of these results is the relatively greater speed of *W* in the simple reactions, combined with lower speed in the more complex ones. Since the greater speed of *H* in these latter reactions does not lie in more open sensori-motor arcs, nor in greater sensory discriminativeness, his results must be referred to more rapid conduction in the higher arcs directly concerned in the organic adjustment to these more complex situations. The independent variability between the simple and compound reactions furnishes strong evidence for the contention that the compound reactions are not merely simple reactions plus such processes as perception, apperception, discrimination, or choice. Hence, the times of these higher mental processes can not be secured, as is so often done, by simple subtraction.

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PRINCIPLES OF SELECTION IN ANIMAL LEARNING

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In comparative psychology, pleasure and pain are generally regarded as the sole selective principles in learning; the successful act is 'stamped in' by the pleasure, and the errors are eliminated by the unpleasantness. In opposition to this prevalent view, this paper will advance certain objective principles of explanation. Before stating these factors, we shall develop several distinctive features of the problem situation which are important from the standpoint of this paper.

The sensory situation which confronts an organism is a highly complex one. In a problem box, the stimulus consists not only of the food, but the problem box, the larger containing box, the table top upon which these are placed, oftentimes the experimenter and various objects in the room, and the motivating hunger stimulus. It is irrelevant whether hunger be due to the presence of a material stimulus or to the absence of a stimulus customarily present. Either sensory condition constitutes a stimulus from the psychological standpoint. Hunger is a persistent irritating sensory condition which arouses activity until some of the acts succeed in altering or alleviating the disturbing condition.

The animal does not react to this complex situation as a unitary whole, as a single stimulus. He reacts to it selectively, and as a series of stimuli. There is a circular interaction between the sensory stimuli and the animal's movements. Each act modifies the stimulus in some respect, and the change of stimulus in turn modifies the act. The motor excess, or the

variability of response to a problem, can be explained to a large extent in this manner. Every movement generates distinctive kinesthetic and cutaneous stimuli. The intensity and direction of the food odor varies with practically every movement. The visual situation differs for every attitude and position of the animal. Cutaneous elements may be added or subtracted from time to time. The successful act introduces gustatory elements and modifies the hunger stimulus. The motor effects of these sensory changes differ in degree and kind; they range from the extreme of strengthening and accelerating the act to its complete suppression due to the initiation of an antagonistic act. What these motor effects shall be depends upon the nature of the resultant stimulus and the animal's organization in reference to it. An act that brings an animal in close proximity to the food is accelerated not because of pleasure but because the animal is built that way. A resultant electric shock will suppress an act through the initiation of an avoiding response. The negative response is to be explained in terms not of unpleasantness but of the animal's innate organization to such a stimulus.

The stimulus to which the animal responds also changes radically from trial to trial throughout the learning process. The animal gradually ignores some aspects of the sensory situation and begins to respond to other aspects which at first were not noticed. In a problem of visual discrimination, a rat reacts at first to the food odor, the exits, the sides of the box —in fact to most anything and everything except the visual objects to be discriminated. He generally makes considerable progress in mastering the problem in terms of position and alternation before his behavior is affected by the crucial stimuli. While the series of responses to a problem are an expression of the animal's innate and acquired organization in reference to that sensory situation, yet these acts are *rarely* aroused by the crucial stimuli themselves. Hence most problems at least involve to some degree the formation of a new association between the crucial stimuli and acts which were originally made in response to other aspects of the sensory situation. It is not our purpose to discuss the method and conditions of forming

new connections. We merely note the fact that they do tend to become established. We have given, then, a system of connections between acts and the various sensory aspects of the problem; some of these connections are new while some represent the previous organization of the animal. The further progress of the learning consists of the preservation of some one, or group of these connections and the elimination of the others.

Selection and elimination are the diverse effects of a single process or mechanism. All connections tend to be preserved; all develop in strength and functional efficiency during the learning process, but their development proceeds unequally. The unsuccessful tendencies are not eliminated in the sense of being torn out by the roots; they are eliminated only in the sense of not being aroused in that situation. The strongest and most prepotent tendencies of the group function first and dominate the situation. The successful act is selected because it finally becomes the most prepotent in the group; all others are eliminated, or better are 'suppressed,' because of their lesser development in functional efficiency.

The problem of determining the various principles of selection and elimination thus resolves itself into a search for those factors which favor the retentive development of the successful act at the expense of the many failures. The principles are relative recency, relative frequency, and relative intensity. We shall develop our conception by applying these principles to the various types of problem. For our purposes, all animal problems may be divided into three classes: (1) that involving a series of simple acts directed toward the stimulus, the final one of which is necessarily the successful one. This type is illustrated by a problem box opened by pulling upon a cord suspended in front of the door. (2) The second type is represented by the maze or a complex problem box involving a fixed series of lever manipulations. The successful act is a complex whole composed of elements which were originally separated from each other by many useless acts. The process of learning involves the elimination of the useless and the serial coördination of the various elements, only one of which can be the final one of the series. (3) The third type deals with the inhibition of some instinct or habit.

All three principles are effective in the first problem. The successful act tends to occur more frequently during the learning than any of the useless acts. The successful act must occur in every trial, while as a matter of fact any useless act on the average does not occur in more than half of the trials. It is possible, however, for some error to be repeated a number of times in one trial. Since the successful act can occur but once per trial, it is thus possible for some error to be repeated throughout the learning process more frequently than the successful act. Why should not this error be selected? Our answer is that this fixing of useless acts does as a matter of fact often occur in both animal and human learning, and the phenomenon supports rather than disproves our conception. As to the final elimination of such useless acts, we are forced to contend that they never would be eliminated on the basis of frequency alone. Their final elimination is due to the coöperation of the other factors.

The successful act is intensified and accelerated by its sensory consequences to a greater degree than is any other act. This act possesses a number of peculiar and distinctive sensory consequences. The string in common with most other parts of the apparatus offers some resistance to attack but its behavior is unique in that it suddenly gives way to the pull. The sudden opening of the door is a surprising and unusual visual and auditory result that not only attracts attention but which often excites a certain timidity. After entrance, the box is sensed from the inside—a rather striking effect to those animals equipped with some disposition either to avoid or to seek such enclosures. The intensity of the food stimulus is increased in a more pronounced manner than heretofore, while the new elements of taste and mouth contact are added. The final and most important sensory consequence distinctive of the successful act is the alteration of the hunger aspect of the sensory situation. That the successful act is characterized by distinctive and important sensory effects is evident from the fact that the only real criterion of a success as opposed to an error or failure depends in the last analysis upon the nature of the sensory effects of those acts. Not only does the successful

act result in novel and significant sensory changes, but there is evidence on the motor side of their stimulative efficiency. The final series of acts beginning with the downward pull on the string represent the maximum of tension, excitement, vigor, decisiveness and acceleration. This change in the character of the animal's behavior is very apparent and striking though rather difficult to measure in any quantitative terms.

The successful act is necessarily the final one of the series—the most recent one. The effectiveness of this factor can naturally be inferred from human experiments. Any explanation of its efficiency is more difficult, but this difficulty obtains for the human realm as well. Although we are concerned more with matters of fact than with explanation, we may mention three possibilities. The successful act stands in a closer temporal relation to the subsequent trial than does any of the failures, and we can assume that the retentive effect of any act is inversely proportional, other things being equal, to its age. This disparity of age, however, would seem to be negligible except in those problems where a number of trials are given in immediate succession. Recency may also be interpreted as temporal contiguity to the food stimulus. In this sense the factor would seem to be a special case of the intensity principle. Again, the successful act may be favored because it is the *final* one, rather than an act which is followed by further activity. It is known that any activity interferes with the gradual setting or fixing of the retentive effects of any immediately previous act. The retention of all useless acts is thus retarded by the necessity for further activity, while the final or successful act is relatively favored by the absence of such subsequent distractions.

The maze is taken for the second type of problem. The effective factors are frequency and intensity. During the first trial the segments of the true path will be traversed more frequently than the cul de sacs, and this disparity must rapidly increase from trial to trial. Let X be a blind alley, interpolated between two segments of the true path, A and B. Let the animal be running forward through segment A leading to B and X. Since the probabilities of entering B or X are even in

the long run, and X can be entered on the forward journey only through A, it follows that A will tend to be traversed twice as frequently as X. What is true for A and X will hold for any segment and its succeeding cul de sac on the forward journey. The same relation will also apply for any returns toward the entrance box. In the latter case, however, the ratio of frequency in favor of the true path will be increased owing to the fact that an animal in these returns very rarely leaves the true path.

The usual distinction between the true and the false paths in a maze is wholly meaningless from the standpoint of a learner, be he animal or human. Such a distinction can arise only as the maze is mastered. From the standpoint of the immediate sensori-motor situation in which the animal is placed, the true path and the cul de sacs are to be distinguished from each other on the basis of the degree to which they impede or encourage the animal's activity. A blind alley is but a sensory obstacle or impediment to the animal's activity; it means hesitation, caution, investigation, or disastrous sensory consequences. The true path presents fewer obstacles; it offers greater encouragement to freedom, continuity, rapidity, and vigor of motor expression. The difference is merely one of *degree*. The blinds check, thwart, and suppress activity more than does the true path, while the latter encourages and facilitates activity more than does a blind alley. The principle of relative intensity is here effective; acts are selected or eliminated according to whether the sensory consequences tend to facilitate and intensify them on the one hand, or to disrupt and suppress them on the other.

For the third problem, let us assume that an animal is reacting positively to some food object sensed at a distance. Let the conditions be so arranged that any contact with the object will result in an electric shock. The animal thus comes to the problem endowed with two ready-made connections, a positive response to the object, and an avoiding reaction to the pain stimulus. We shall term these connections $S–M$, and $P–A$ respectively. The functioning of the first tendency inevitably results in the arousal of the second and the two are

brought into conflict. This second disposition is necessarily the stronger or it would not dominate in the conflict. In this situation, the second act A becomes connected with some sensory aspect X of the object, and the new connection X–A is formed. The only essential requirement of this stimulus X is that it be sensed at a distance. The further task of learning now consists in the development of X–A to a point where its functional efficiency is greater than that of S–M. Its strength does not need to exceed that of P–A, because the successful functioning of X–A will prevent the animal from receiving the pain stimulus. All three of our principles favor the connection X–A over that of S–M. The avoiding response is the more recent. The retentive development of the positive response is interfered with by this violent inruption of the pain stimulus and its motor results. The latter disposition is, however, subjected to no such distractions. The negative response is the more intense and vigorous one. Because of its greater recency and intensity, the resulting disposition is so susceptible that it is likely to be aroused and become connected with almost any opportune stimulus connected with the total situation. What this stimulus shall be and the number of trials necessary to establish an efficient connection will depend upon the nature and intensity of the possible stimuli and the organism's capacity to sense them. After the new connection approximates the old in functional strength, the factor of frequency will become operative and assist in the final stages of the elimination.

In considering the efficiency of these principles, possible critics must consider that they can work in coöperation, and that their effects are also cumulative from trial to trial. Neither does it follow that this cumulative effect proceeds according to an arithmetical progression. Neither do we contend that these are the *only* possible principles; the theory makes no pretense of completeness and self-sufficiency. These principles are also designed to explain the *mere fact of selection*, why an act with certain attributes invariably survives and why all other acts are eliminated or suppressed. There is no pretense of an explanation of *all* aspects of the learning process.

We emphasize this point because we wish to avoid the charge that our set of principles is obviously incomplete and inadequate to an explanation of the learning process as a whole. To illustrate, it is known that the 'temporal distribution of the trials' is a very important condition of learning in both the human and animal realms. But this factor influences merely the *rate* of selection; it determines in no way *what* acts are selected and *what* acts are to be eliminated.

Our conception may be briefly compared with the algedonic theory in several respects. The algedonic theory generally regards pleasure and unpleasantness as the sole principles of selection. We deny this adequacy in both the human and animal realm; we deny the assumption that all eliminated acts are unpleasant and that all surviving acts are pleasant. Such a conception does not allow for the existence of neutrally toned activities. Many useless and trivial acts are frequently selected. Acts both useful and useless may become fixed and the learner may remain in blissful ignorance of their very existence, to say nothing of being aware of their affective character. Many acts are learned which are distinctly unpleasant during the early stages at least. Pleasant activities often fail to reach the habit stage. With our conception these anomalous cases are readily explicable.

In the animal field with which we are specifically concerned, the algedonic theory meets the difficulty of an objective criterion of these subjective conditions. Obviously these selective factors must be validly inferred from certain aspects of the objective sensori-motor situation. Some discussions seem to ignore the issue. Others apparently commit the fallacy of utilizing the phenomenon of selection itself as the criterion of pleasure. This is a case of reasoning within a circle. The selection of a certain act is observed and noted. The conclusion is reached that this act was pleasurable because it was selected, and after the existence of pleasure is thus determined, the further conclusion is advanced that the act was selected because it was pleasurable. Such an explanation is merely verbal and any alleged knowledge obtained is purely chimerical. Any valid algedonic theory must choose as its criterion of

pleasure some fact or aspect concerned with the learning process other than the fact of selection itself. Moreover, this fact or aspect which is to be utilized as the guarantee of the existence of pleasure must be one which can be observed, studied and described without any reference whatsoever to the fact of selection. The difficulty of determining such an objective factor and of securing any general agreement upon its validity is readily apparent. Moreover, the law of parsimony would suggest that this objective factor be utilized as the selective agency itself rather than as a mere index of the causal principle.

Any theory is in duty bound to attempt some rational connection between its explanatory principles and the phenomenon of selection. The algedonic theory meets difficulty in this respect. The affective processes must be so conceived that they can exert some causal influence upon the sensori-motor activities involved. We do not contend that this difficulty is necessarily insuperable. We merely refer to the fact that no conception has won any general acceptance. Moreover, the algedonic theory necessitates a double mechanism. Pleasure must increase retentive development, while unpleasantness must check and decrease it. Elimination is thus not a mere matter of suppression but a tendency which operates to tear out connections by the root. In opposition to these difficulties, our conception offers a single mechanism for both selection and elimination, and one which is in harmony with generally accepted principles of psychology.

Supporters of the algedonic theory may admit our positive contentions, but urge that there is no necessary antagonism between the two views, that the two can and should be combined. Selection would thus be determined by the recency, frequency and intensity of the pleasure. Such a combination seems plausible at first thought, but a careful analysis will show a combination is impossible. Either the affective processes are the sole selective principles and the objective factors play the subordinate rôle of determining the *rate* of selection, or the objective factors are the real selective agents while the affective elements are mere useless luxuries having no function whatsoever. The situation offers no opportunity for the straddler.

CERTAIN FURTHER FACTORS IN THE PHYSIOLOGY OF EUPHORIA[1]

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CONTENTS

I. Introduction

In a recent article (5) on the relations of sthenia to certain phases of education, the writer had occasion to mention, and a little more, some of the factors of physiological euphoria (experience of satisfaction or of pleasantness) as set forth, for example, by H. Spencer (25), Marshall (20), Max Meyer (21, 22), myself (9) and many others early and of late. The essence of all such discussions seems to be the ample, unimpeded, undeflected, and furthering nervous impulses from large receptive fields especially such of these as represent personal expansiveness. It is our present endeavor to make slightly more explicit certain of these receptive fields, loci of personal relation to the effective environment.

One matter of definition of terms must be understood at the outset, as all along: "euphoria" is not necessarily coextensive with happiness or even with "the experience of satisfaction or of pleasantness" for either of these may be, and of course frequently is, dependent on ideational processes

[1] From the twenty-second annual meeting of the American Psychological Association at New Haven, 31 January, 1914.

proper, in the narrow sense, that is, of conceptualization: a mere idea, a pure idea may make a man, for the time at least, happy and afford him what we might well learn to call *ideational* euphoria as in some degree different, especially in its occasion, from the euphoria of which we write—physiological euphoria. It should "go" without being said that in the long run the former has dependence on the latter; else, indeed, all of us more or less were poets, artists, metaphysicians, reformers, and science had no place. In choosing a home-site a large library or even a prospect sublime as the skies is not enough. Our concern in this immediate article is then with euphoria in its dynamic or physiologic relations. We shall discuss some of the bodily and environmental conditions that seem most importantly connected in and with physiologic euphoria under normally biologic circumstances, the interesting euphoria of insanity being, for the time, ignored. Advancing understanding of certain physiologic relations makes further psychologic analysis now possible, and provides us with more cenesthetic "mind-(?)stuff" as the receptive fields become better and better understood.

It is a traditional presumption worthy of all acceptance that the affect, the emotional balance whether euphoric, dysphoric, or neutral represents the personal reaction when the individual is more or less well adapted to its environment, especially its organic environment. The individual as a whole and neurally has some "attitude" toward non-individual part of his experience. It is important to note that for neurology a part of this objective environment may be within the body of the individual, anywhere in short except within a nerve-center! For convenience, then, in this particular discussion, and perhaps elsewhere, we may divide a man's environment into two parts equally essential almost to his behavior and to his well-being, 1, outside the skin altogether and, 2, outside the receptors in the viscera, muscles or wherenot in the body. So far as neurility is concerned obviously these are equally environmental. (Such a consideration is only one more step in that integration of life and mind and environment which is a key-note of our day.) That in the long run and in general the affect

represents more truly than anything else the personal reaction to environment thus broadly denoted need not be enlarged upon here. Verworn in his Yale lectures published under the title "Irritability" (26) has recently made an elaborate analysis of the bases of this interdependence and concludes, rightly and usefully it appears, that "a stimulus is any change in the vital conditions." This is a good step beyond Fechner and Du Bois-Reymond and in the right direction of orienting personality and its determinants.

We seek, therefore, to make a bit more definite and concrete the current notions as to the euphoric receptors as receivers of changes in the vital conditions and to suggest, if we may, the general nature of the afferent influences that determine the tonicity or strain or neurokinesis or whatever in the central and especially in the cerebral, gray. Such a discussion is in a sense and degree the homologue on the one hand of a sketch of the neurology of voluntary movement already proffered (11) and, on the other, of the neurology of the vegetative mechanism to come as soon as the autonomic system, especially its afferent aspects, now so blind, shall have been revealed. Thus little by little behavior's neurility will be elaborated as a truly adequate "sanction of psychology."

II. Possible Further Euphoric Factors

It appears that three factors especially of the influences of the euphoric receptive fields may be profitably described in somewhat new detail. Each of these is complex enough and as yet little enough understood to challenge the curiosity and the scientific constructive imagination of many investigators. The three factors noted are: *A*, nutritional and sympathetic influences from the active intestinal villi; *B*, kinesthesia; and, *C*, the epicritic impulses. Let us examine into the part each of these obvious factors takes in euphoria; it seems to be another troublesome case of estimating our income of life-satisfaction at the source. It is clear that ideational euphoria and the ideational elements of euphoria in general arise more directly from the cortical associations, yet these latter, too, recent observations suggest, secure their animus in the environ-

ment to some extent and largely in the innervation of expression-muscle be the view that of animism or other.

A. The nutritional and sympathetic *influences from the intestinal villi* are clearly two things and not one. The nutritional influences toward good humor, "feeling good," go to the neurones (and especially to the cortical nerve-cells?) in the portal and systematic blood-streams; while the sympathetic impulses are the little-known but certain afferent autonomic nerve-currents which experimental physiology and one's personal experience both suggest. We may dismiss these neural contributions to the cenesthesia of comfort and well-being with the above word,—but of necessity, not from choice, for it may not be doubted that these afferent impulses from the viscera have much of importance to do with the determination of moods and passions and temperaments, with, in short, all the most fundamental affective themes that underlie consciousness and behavior.

The nutritional influences toward good humor are not as yet on a basis of physiological demonstration through vivisectional experiment and chemical analysis of the blood in euphoria and in dysphoria. Such demonstration would be difficult to carry out owing largely to our necessary ignorance of the affective tone of a brute when not part of a definite emotion such as fear or love. The affect during a fast deliberately carried on for days or even a few weeks is a problem apart, and one which merits study: in some manner, much perhaps as in a bad pneumonia or other asthenic fever, the weakness here merges into a relative anesthesia in which the affective tones both ways are indistinct. But under ordinary conditions of nutrition there is undeniably, I think, a demonstrable direct relationship between absorption from the small intestine and the affective tone. The dysphoria of acute fatigue, in part certainly central, would otherwise not be so immediately relieved by a glass of hot milk or malted milk or of some variety of soup. A hungry worried man home from the office to dinner could not feel his dysphoric worries slip away so very quickly from any other influence than a direct nutritive stimulation of the central, especially cortical, nerve-cells. It is not a traditional delusion

that fat men and boys are usually good-natured and lean women cuttingly keen and not obviously too happy. On the one hand the Esquimaux and on the other the races of southern Europe, both eaters of much fat, certainly have a higher euphoric index than the Scotchman, for example, or the thin down-East Yankee.

While the exact chemical nature of the Nissl's granules (tigroid substance, chromatophile bodies, chromatin bodies) is not definitely known as yet (owing to the minuteness of the nerve cells and the impossibility so far of separating thees bodies from the cytoplasm around them), there is general belief that this material chromatin is a complex substance essentially compounded of fat and protein in which the characteristic determinant is what the biologists term a lipoid, a "fat-like" material, phosphorized fat (Goldscheider). Austin and Sloan (1*A*) and Dolley (12) have demonstrated the direct dependence of nerve-cell activity on the chromatin masses, and the quick loss of chromatin when the katabolism exceeds the anabolism. I think we may, then, deem the cerebral end of the process under discussion well established:—that the normal action of the cortical-neurones requires an abundance of circulating fat (or "lipoid"-producing carbohydrate) as well as of protein. In addition to the chromatin-masses, the myelin sheath of the medullated nerves is an adipose substance, very likely nutritive to the neurite.

So far as the competency of the circulation is concerned to keep the minute nerve-cells in immediate and constant relation with the blood-plasma (lymph), it is enough to remind the reader that a blood corpuscle passes entirely through the systematic and the pulmonary "circulations" in about thirty seconds; that the capillaries are everywhere and with walls so permeable that they exist, so to say, only for the red corpuscles; and that the protoplasm of the human cortex is about 85 per cent. water. The temporal unification then of nerve-cell nutrition and the portal blood (from the intestine) is surprisingly complete; an increase in the fat-content in the thoracic duct would be almost immediately used in the cortex, raising the metabolic tonus of the nerve-cells to a better affective concomitance.

Passing now to the mechanism and process of intestinal absorption we see in the neuro-musculo-glandular mechanism of the villi and the valvulæ conniventes a much more competent and actively adaptable apparatus than has been disclosed heretofore. In general terms the presence of muscle means the active adaptation of an organ to conditions outside itself, for example in the absorptive organs mentioned as well as in the spleen and ovary.

Whatever may be the adaptive movements of the valvulæ, the villi are the chief immediate organs of food-absorption from the intestine. There are about four million of these organs in the human. They are irregular but in general finger-shaped organs varying in length from 0.5 to 3.0 millimeters. Their combined surface area situated as they are on the valvulæ conniventes increases the absorptive area of the intestine at least an hundred fold over what it would be were the gut a smooth-walled tube instead of one partly filled at times by these organs. The villus is a complex little organ for, besides its versatile and essential wall of columnar epithelium, it consists of smooth muscle, autonomic nerves, a conspicuous central lymphatic ("lacteal"), blood-vessels, leucocytes, and connective-tissue. It would be pedantic to venture conjecture as to the exact modus operandi of such a mechanism. Fat, however, is the only alimentary principle which is mechanically conveyed from the midst of the epithelial cells (where it is apparently synthesized from the fatty acids and glycerin) to the central lymphatic. The chief function of this "lacteal," so far as known, is to receive the fat globules and to forward them into the circulation proper via the thoracic duct. Howell says (1913): "The mechanism of absorption remains unexplained." It is, however, extremely probable that the neuromuscular mechanism of the villus has, as part of its function at least, the compression of the villus under nervous requisition for more fat from other parts of the body. On this basis the villus is understandable as in part a minute reservoir of adipose material, perhaps indeed chiefly for the greatly variable use of the nervous system, nerve-cells (chromatin) and nerve-fibers (myelin) alike. They clearly make up by their number what each lacks in size.

We have now before us the vitals of a mechanism, receptors, adjustors, and effectors all complete, by which the nervous system may secure fatty (and protein?) nutriment suitable to its wide range of requirements in rest and activity and dynamogeny.

It may well be taken practically as an axiom for the relations of mind and body that optimum physiologic and psychologic function represents, in the long run at least, physiologic euphoria; at any rate we are basing this metabolic suggestion on this presumption, and probably without protest.

B. Kinesthesia is the second kind of contribution to be noted to the neurility of physiologic euphoria. The status of this, the fundamental behavior-sense, to so crudely term it, has been of late set forth by the writer both in its physiologic (7) and its efficiency aspects (8, 11). Suffice it then to say here that thousands of afferent impulses, strains, or influences from as many receptors in the joints, muscles, tendons, skin, and bones are continually pouring into the psychomotor centers. These represent in the ultimate analysis the *environment* to the personality within and, more specifically, integrate the body and the mind, furnishing to the psychomotor centers their only data by which the body may be coördinated.

From the somewhat different view which is rapidly becoming understood and accepted (like subconsciousness and the new-mind-stuff theory and the expedient continuity between protoplasm and mentality), usually denoted as the reservoir-idea, kinesthesia supplies a considerable fraction of the intake of this head of neural energy. Muscle of course always, even in deepest slumber, has some tonus and always therefore, together with its mechanical fellow-tissues just mentioned, is sending floods of energy into the central nervous system from the latter's environment. This gray-reservoir idea has not as yet been universally enough accepted by psychologists; yet nothing is more certain than that in some mode it exists, and its usefulness to psychology every man may read.

The precise relationship between euphoria of the physiologic type and the quantity of this kinesthetic flood representing the body is probably not a direct relationship at all, for else

conditions of the greatest bodily activity (short of cramp or catalepsy or general convulsions) would be those of the greatest delight (in so far as affect and not sensory pleasure). This of course is not the case in a bald and strict sense. On the other hand, the present writer believes that it is more nearly true than would be generally supposed at first glance and that the observed discrepancy can be explained by extraneous circumstances such as distraction of attention without, etc. In certain forms of general activity represented by kinesthesia to the brain, the relation of the quantity of movement to the euphoric index is obvious; in other phases of neuromuscular activity the direct correspondence cannot be directly made out.

Ribot in his latest book (22*B*) has set forth in a most interesting and important way the utter dependence of the subconscious aspect of mind on kinesthesia. And the neural determinants of euphoria are of course inherently subconscious. (This book of Ribot's, like his others, should not be overlooked by any one who would understand the bases of behavior, for they are as fundamental as they were prophetic in modern psychology.)

On the whole, so far as the mere amount of euphoric kinesthesia is concerned the correspondence seems certain but often vague.

In bodily activity in which the kinesthesia is intensively conscious and widespread the direct relation appears in emphatic form. This condition of intensive inhibitory consciousness we commonly designate as grace or gracefulness, and it is clearly a generalized skill. The logical limit of grace in the human active body would be in a form of exercise forever denied to man, namely flying. Swimming, skating and real (classic) dancing appear in practice as the maxima of grace, then, and these, as arranged here in an ascending series, seem to be, other things equal, conditions of physiologic euphoria.

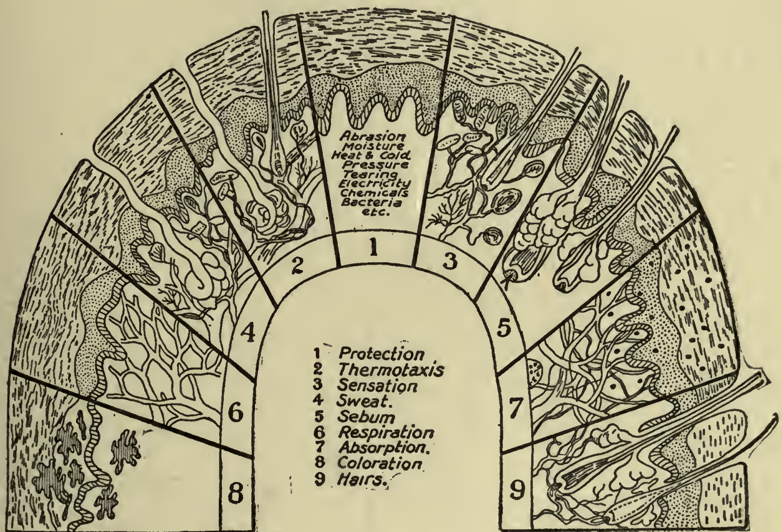
When (to take a third case and phase) the kinesthesia is localized but all the more highly intensive and inhibitory and conscious on that account, we have a kinesthetic euphoria arising in any form of what is commonly termed skill. A sleight-of-hand performance, guiding a fret saw, engraving on

metal or carving wood, drawing, pitching skillfully a base-ball —all such movements have an inherent pleasantness essentially distinct from the other factors which take part in the total behavior. They supply in intensity of kinesthesia what they lack in quantity of ingoing stimulation. From this kinesthesia comes in part the euphoria in general of all creative motor processes; without it not even the one-in-a-million child would ever learn to play the violin within the limits of human toleration.

Ample, unimpeded, undeflected, and furthering nervous impulses, then, from the action system seem to be an important factor in the euphoric neurility whenever the neurones are adequately nourished.

But kinesthesia has more to say in this matter, probably, than to emphasize *itself!* Bergsonite at least to this extent, I am convinced that in as much, in so far, as kinesthesia does, as already suggested, represent the environment to the personality, every sense owes its experience of intensity and of extensity to kinesthesia. Sensation proper has only quality it really seems, and whatever of quantity or intensity it represents in experience or in fact it derives from the spatial (bodily) aspect of our two-aspected personality. It is bodily reaction (as consciousness, what but kinesthesia?) which supplies the extensity, as well as the intensity, to sensation. Years before the French light [Bergson and (22*B*)] dawned upon my physiologic ken, the absolute necessity of recognizing how inherently implicated space (and all its relation) in our personality, was made explicit in certain extravasations on voluntary movement (11). The body represents environmental space (as already defined) just as kinesthesia represents body and physiologic euphoria represents kinesthesia. In this nexus of inevitable experience only does the soul of man get its insistent purposes revealed—whether fulfilled or not, at any rate revealed; only so, then, does it reach reality and self-hood. Of this personality euphoria assuredly is an important aspect. The far-reaching meaning of this relation between space and experience has not as yet been adequately applied to the becoming science of behavior, the science of "things as they are."

C. The third suggested factor in the euphoric cenesthesia, noted above as *the epicritic impulses*, is even less readily described with scientific satisfaction because, like kinesthesia, the human skin is a relatively neglected field for intensive study by the histologists and physiologists. It appears that stains more clearly differential than those now in use are needed before the anatomic basis of the epicritic influences and the many factors of this complex and important organ can be made fully known. Few save certain specialists in biology realize how complex this simple-looking mechanism really is.



The accompanying diagram, from the writer's textbook of physiology (4), shows the various functions, but, like all other pictures at hand, fails to suggest adequately the varieties of receptors,—in this case the chief interest.

A list of the so-far-discovered more or less influential elements of the human skin would include the complex vasomotor mechanism, afferent sympathetic dendrites, the peculiarly efficient epidermis, sweat-glands, sebacious glands; Meissner's corpuscles, the terminal cylinders of Ruffini, tactile menisci, the nerve-rings of Bonnet (?), Vater-Pacinian corpuscles, free nerve-endings, heat-receptors, cold-receptors, pain-receptors,

pleasure-receptors, perhaps tickle-receptors; arrectores pilorum muscles with their coördinating nerves.

The pioneer uncoverers of all these things (Goldscheider, von Frey, etc.) we do not for our present purpose need to refer to, but certain recent observers and writers have emphasized the completeness of the *integration* of these dermal functions. Thermotactic physiology has long realized one phase of this unification, but other phases have been more recently developed by Head, Sherren, and Rivers (15, 16), and L. Hill, etc. (18), Sewall (23), Crile (3), and others. The almost heroic work of Head in this direction is a fine contribution to our knowledge of dermal cenesthesia but important as it is for psychologic physiology has less immediate use for us now than the suggestive papers of Sewall and of Crile combined with other inductions of those hygienists who are working on the science really underlying ideal ventilation.

For one thing, evidence accumulates that one large and well-nigh indispensible element of euphoria is stimulation of the skin in the way primevally *natural to it*, and in such a way as to allow of the free action of its combined receptive fields. The dermal receptors appear to be tuned to act euphorically, that is normally, only together, not separately nor in generic groups. The fields in short of the dermal sense organs are really receptive *fields*. Just as a spring wind blowing over a rich natural meadow would beneficially influence all the different kinds of herbage at once to the general enrichment of the field, making all of it glad, so, somewhat, here. The simile indeed is more than a chance choice, for it is to just this matter of moving air, outdoor air even when indoors, which I would call attention now as a basal factor in physiologic euphoria.

The empirical facts, at least, become more obvious if we state the matter in its negative aspect: We all realize that air that is "dead" (*i. e.*, not moving) (Hill, *loc. cit.*); humid and too warm; humid and too cold; or lacking in oxygen; is a ready occasioner of organic dysphoria. We are here not concerned with pathological conditions, but it is relevant at least that the still blind complex we designate as chill in some way dominates the skin's euphoric balance and indeed that of the

entire cenesthesia or sensation-fabric. Ill-defined irritations in the abdominal or thoracic viscera (especially it seems in the lungs and the intestine), and a rapidly rising body-temperature from whatever cause, almost at once can change a distinct euphoria to dysphoria at once as deep-seated and as superficial as receptors are to be found, but dominant especially of the dermal affect. It would be pedantic to claim this ill-comprehended state of chill as neurologically a lack or a set of lacks, but to suppose the normal conditions mentioned at the beginning of this paragraph lacks seems wholly justified by over increasing real knowledge of the relations of the skin to its environment. Neurally then, dead air means a lack of movement over the skin; air that is humid and too warm means a lack of stimulation by the optimum temperature and by evaporation; air that is humid and too cold means similarly a lack of the optimum temperature and a lack of dryness, evaporation so being hindered with its own important neural lack; lack of oxygen in the air, whether from its general chemical composition or from its utter deadness next to the skin, means an obvious lack of receptorial stimulation that we shall briefly consider anon. The whole trend of recent opinion, notably Sherrington's (24), is to suggest how minute, both qualitatively and quantitatively, is the adaptation under different circumstances of the receptors, especially those of the cenesthesia. Indeed Sherrington's conjectures along this path have opened a first way for useful working hypothesis on which to proceed as to the viatility of the spinal gray. Lack of movement, optimum temperature, dryness, and oxygen certainly mean lack of stimuli for very numerous receptors and a consequent lowering of the cenesthetic tide. Neurologically they seem to represent the absence or reduction of multitudes of neurokinetic contributions to the tonus of the areas of the cerebral gray, and therefore a diminution of the environmental energy representing conscious well-being. The "head" or pressure in the dynamic "reservoir" is lowered or alienated or opposed and normal euphoria thwarted.

The careful tuning, so to say, of the many and many kinds of receptors, is a matter worthy of extended research in

the laboratories. It has been already virtually discussed to a slight extent in descriptions of the heat- and cold-sensations and in the conjectures as to their continuity or discreteness, etc. Indeed these particular receptors show what I have called tuning in an especially clear way. Evidence accumulates, however, that other sense-organs, those of oxidation, of evaporation, of tickle perhaps or of touch, may be in like manner if not in like degree relative to extraneous conditions and—tunable. It must be remembered that the dermal senses, like smell, in the human are distinctly decadent senses (save in the fingertips, tongue, etc.), because so unnaturally (from a biologic standpoint) protected from wind and cold and sleet and storm, from especially perhaps the oxidation, local and reflex, for which these environmental modes of energy stand in the central nervous system. The conscious aspects of these receptors have apparently degenerated in the centuries that have gone, but the neural influence, especially in so fundamental a relation as that of the algodonic balance, remains unchanged.

Says Ellis (13) "The hygienic value of nakedness is indicated by the robust health of the savages throughout the world who go naked. The vigor of the Irish, also, has been connected with the fact that (as Fynes Moryson's *Itinerary* shows) both sexes, even among persons of high social class, were accustomed to go naked except for a mantle, especially in more remote parts of the country, as late as the seventeenth century. Wherever primitive races abandon nakedness for clothing, at once the tendency to disease, mortality, and degeneracy notably increases, though it must be remembered that the use of clothing is commonly accompanied by the introduction of other bad habits. 'Nakedness is the only condition universal among vigorous and healthy savages; at every other point perhaps they differ,' remarks Frederick Boyle in a paper ('Savages and Clothes,' *Monthly Review*, Sept., 1905), in which he brings together much evidence concerning the hygienic advantages of the natural human state in which man is 'all face.'" Of these 'hygienic advantages' euphoria certainly takes no small part. Says Sewall (23): "It is through the genius of the moving air of the open that the temperature

nerves find their most salutary stimulus and induce metabolisms characteristic of highest machine efficiency in the body. The physiologic difference between 'open' and 'closed' air depends partly or wholly on differences in the stimulation of the temperature nerves of the skin under the two conditions. The environment of the open air is conducive to an esthetic state that should not be ignored as an aid to healthy living." Indolence has been preponderatingly active among the savages of the tropics, but who can doubt that herein is one of the factors in the predominance of the man and the woman in the earth's temperate zones where the air is cool and stimulating, helping to make them happy as well as healthy,—closely related states.

A biological consideration is worth suggesting: The skin was the primeval receptive field in the phylogeny as still in the earliest months of the ontogeny, so that subconsciousness relates gentle stimulation, especially of optimum temperature, gentle friction in particular, to euphoric states. The reader does not need to be reminded that every known animal of sufficient evolutionary grade acts as if it enjoyed gentle massage of its skin. Closely related is sexual "contrectation" (Moll) which explains the seductiveness of the caress in its relation to the euphoria of being loved—the psychophysiology of which the writer has recently developed for the School of Eugenics.

The matter of the pleasantness arising from the movement of the environmental air even when a human body is clothed as is customary in English laboratories is vividly illustrated by Hill (18): Eight men were confined in three cubic meters of space; the temperature was 87° F., the content of carbon-dioxide 5.26 per cent., and of oxygen 15.1 per cent. "The discomfort felt was great, all were wet with sweat, and the skin of all was flushed. The talking and laughing of the occupants had gradually become less and then wholly ceased [dysphoria]. *On starting up the electric fans* the relief was immediate and very great," (euphoria) in spite of the rising temperature. No headaches followed or other after-effects. In another experiment carbon dioxide up to 2 per cent. was forced into the box unknown to the eight subjects without being noticed at all by them. Most of us have had essentially

similar experiences on sultry July days when a breeze has suddenly sprung up. The pleasantness then is a very real and physiological experience.

Professor Wm. G. Anderson, of Yale, has recently published (1) the report of a virtual repetition of Hill's work (18), and is inclined to conclude that a great increase in the proportion of carbon dioxide (from 20,000 to 30,000 per cent. above the outdoor normal of about three parts per ten thousand) is an important feature in the causation of the distressing features of a lack of ventilation. How could it be otherwise? The subjective reports of conditions as experienced or felt suggest that the feeling of dyspnea is distinct from that arising in the skin, for the starting of the fans in the close chamber afforded relief but the mild feeling of suffocation was not lessened.

One subject (a medical sophomore) reports "slight headache relieved by the fan"; another: "feeling of warmth and perspiration which was relieved by the fan;" another: "fan turned on, felt cooler and headache disappeared;" another: "discomfort, which was slightly relieved after the fan started"; another subject, the experimenter, "No symptoms except slight dyspnea [but] fan relieves the feeling of heat and wetness, but not dyspnea. After these tests a slight sore throat."

In another experiment with temperature of 83° F., relative humidity 43, carbon dioxide 8.10 per cent., despite the starting of the fans which "relieved the closeness and warmth," (ten adults were in the cabinet) the suffering from the dyspnea persisted: "the sense of oppression was such that the women felt as if 'they should scream or faint' and the men were quite ready to force open the doors." For our psychologic purpose, the subjective reports of the subjects in this part of the work are perhaps worth reporting:

Dr. R.—"Dyspnea, slight headache, relief from heat when the air was agitated but no relief from dyspnea; no permanent bad after effects."

Dr. M.—"A very slight headache while in the room and for 30 minutes after, but normal the following day."

Dr. H. L. A.—"At the end of thirty minutes very much depressed, felt weak, dyspnea, headache both sides, parietal, felt crowded, mental depression. When all left the cabinet I experienced mental and physical relief. Headache lasted three-quarters of an hour."

Miss P.—"Dyspnea, headache, depression, suffered with coldness and had slight chills for half an hour after leaving the cabinet. All right in the evening and next day."

Dr. B.—"First noticed warmth, then dyspnea and flushing of the face, throbbing of the temple arteries later, distress in epigastrium, headache which became worse on leaving the room and lasted all day. Also slight dizziness and slight mental impairment."

Dr. H.—"Frontal headache, flushed face, some perspiration, symptoms of a cold increased. Following day no bad results in evidence."

Dr. S.—"Perspiration profuse while in the cabinet, chilly afterwards, dyspnea after about five minutes, sick in the stomach, headache, micturition frequent for four hours. All rest of the day, evening and night head felt full, could not use microscope. Felt well next day when out doors but 'stuffish' in the laboratory."

Miss D.—"Had chills and other mental and physical disturbances similar to those mentioned by Miss P."

Dr. W. G. A.—"After leaving the cabinet there was severe pain in the head, temple and eyes. Some difficulty in seeing and a very evident uneasiness. Walked home, felt nervous. Ate usual light lunch. Attacked by dizziness at 1.15 p. m. (The blind headache of my boyhood.) Treated these symptoms and reported at Medical School at 2 p. m. Could not see the figures on the gas apparatus. No after headache but a heavy feeling. Vision normal at 2.30, no return of blindness, but the after effects of poisoning were present until 9.15 p. m.

"When fatigued one particular tooth aches, and certain muscles twitch—biceps, deltoid, etc.—but on this occasion the flexor pollicis twitched for ten minutes and the molar ached or was sensitive for three hours.

"When poisoned by digestive toxics I have blind headaches. When fatigued by overwork I am apprehensive and feel that much is to be accomplished that is not done. The CO_2 (?) produced this state of mind. In other words, fatigue of mind and body and CO_2 poisoning affect me alike. There was no rise of temperature and no chill. There is some slight nose-bleeding." (This subject was the experimenter.)

Professor Anderson's remark that "fatigue of mind and body and CO_2 poisoning affect me alike" is interesting especially in the light of Verworn's theory of fatigue (26) as essentially a suboxidation, for there is much evidence to indicate that carbon dioxide is less poisonous to man than the lack of oxygen is depressive and fatiguing. On the whole, seen broadly, Anderson's work seems to corroborate Hill's conclusions if we construe, as is needful, lack of oxygen in place of excess of carbon dioxide. In any event, Hill's findings as to the great relief in the dysphoria afforded by atmospheric movement are upheld, and no one could expect that anything at all would long compensate an organism, especially one as psychophysically complex as man's, for a deficiency of this inevitable factor of our mortal environment, oxygen.

The fact seems to be, then, in general, that more or less regardless of the composition, temperature, humidity, etc., dead air against the skin tends to be dysphoric in action; while air moving over the skin is one of the most important of all the factors of euphoric stimulation and of sthenic activity. So far as this mental aspect is concerned the temperature of the air, however, seems to be a determining quality, in the sense at least that air (or water) too cold or too hot turns the balance to dysphoria (chilliness or sultriness). Because in part of its fundamental importance in ventilation (see (6)) and in the science and art of clothing and in the hygiene of the bath, the dermal problem suggested here is not only a matter of large practical importance, helping to fix the conditions of our greatest practical health and efficiency, but it is also a nice enigma for solution by psychology—for the crux of the problem is psychological; to be carried out, as usual, physiologically. The present writer makes no present pretenses certainly to have solved the problem, and it is with much diffidence (sic) that he ventures the following suggestions. Real solution waits on histology or on a new kind of histologic physiology, whereupon psychology may determine the euphoric relationships.

Of the several possible explanations of the empirical facts now roughly stated above, two, taken together or possibly separately, seem the most likely in view of what is now known about the skin and its stimuli. One of these is a process of *evaporation* and the other *oxidation*. One or both of these neurally physiochemic functions seem to stand for the necessary floods of (euphoric) neurokinesis into the central nervous system.

The evaporation of the sweat poured out into the epidermis, a sponge-like reticulum of keratin, is well known to be the chief means to the regulation of temperature, in homotherms. The average daily amount appears to be about 1,500 c.c. but a group of glass-makers observed by McElroy had an average secretion of about 25,000 c.c. in the course of a nine-hour "day"; occasionally (owing to some bulbar trouble?) the production stopped, whereupon the man would become ill,

have to cease work, and would be revived by the active efforts of his fellow-workers. These facts are cited because they show the very great and useful adaptability of the sweating function and the extreme dysphoria of its cessation. Sultry and "muggy" weather shows us the same thing of course, unless we be exercising actively when in part the freer evaporation reduces the dysphoria even to its opposite, to a tone of pleasantness. One sees, for example, the students in a summer school of physical education enjoying with a real euphoria vigorous exercise with the gymnasium-temperature in the nineties.

It is not then by any means obvious that the "cold-spots" (we know nothing as yet of the actual receptors) are the sole forwarders of the streams of energy that represent euphoria due to dermal or sudoral evaporation. Several other of the receptors noted above may be concerned in this process of adaptation of the individual to his environment through energized tones of pleasantness and of unpleasantness. Here the case is homolgous, biologically speaking, to two sensations, whose end-organs are no better known, pain and pleasure.

The second process which appears to actuate dermal receptors so as to effect an euphoric tone in the individual's consciousness (not to say in his subconsciousness) is oxidation, one of metabolism's foundation-stones. Experiments done long ago seemed to show that so far as the body's respiration is concerned only about 0.5 per cent. occurs directly through the skin. But this small fraction shows that oxidative processes do occur in the skin. When one considers the minuteness of the various dermal receptors and their possibilities of actuation by the "circumambient air," together with physiologic data immediately to be noted, the reasonableness of supposing dermal oxidation to be a factor of euphoria is readily admitted. Bohr (2) showed that ventilation of the blood in the lungs is probably a reflex process of active secretion by the alveolar epithelium. Y. Henderson (17) on the other hand, while admitting the oxidative secretion, supposes that the depression of "mugginess" comes from the kolionic inhibition of this secretion in the lungs. The receptors of this oxidation reflex it is possible, or rather more, are in the skin, and may be found to be one of

the varieties of end-organ mentioned considerably above. Lusk (19) showed by experiments in which men were emersed in water at 10° C. for from seven or eight to twelve minutes that the metabolism increased 181 per cent.—and respiration is always the metabolic index. The experimenter ascribed the increase to the men's shivering, but it seems possible at least in the light of Bohr's work that the increased activity of alveolar secretion of oxygen into the blood may have something to do with the heightened oxidation.

This supposition seems strengthened by late work of Verworn (26) which demonstrates, among other important things, the immediate dependence of the action of the nervous system on oxygen,—an extension of his much earlier proof that ameba stops flowing in about an hour when oxygen is removed from its environment. Without idle speculation as to the affective tones of ameba (!), it is fairly rational to presume that some or all of the delicately complex receptors in the human skin, close to the air as they are, may have their activity and their consequent streams of neurokinesis increased by exposure to moving air as contrasted with air that is dead. It is my present hypothesis, then, that moving air in some way has a tonic action on the afferent influences from the skin by stimulation of whichever receptors in that very complex receptive field are tuned to this mode of energy. The mere presence of oxygen is not enough for a normal euphoria—it actuates, perhaps by way of the pulmonary epithelium, only when coming as a moving force (with friction perhaps) against or over the skin. If, however, friction be really an element in dermal cenesthesia, it is probably not the gross mechanic friction one is apt to think of first, but rather a subtle sort of physiologic friction, so to say, adapted to the extreme delicacy of the organic instruments so abundant in the human skin. On the other hand, the mysterious highly euphoric stimulation of a gale of wind when not outside the optimum range of temperature (as in the Nova Scotian summer-land in September) is known to all, and this implies that gross friction, friction in the ordinary physical sense of the term, may be also a factor in the experienced product. Massage and the caress seem to possibly imply the same thing.

If such be elements in the centripetal cenesthesia related to euphoria, we logically should seek to discern what becomes of the various neural influences as they enter the brain. This we cannot do as yet with perfect confidence in any further detail than has been already set forth, *e. g.*, in (5). The affective tones appear to be somehow related to the thalamus and to the great cortex especially its upper "layers" perhaps. Southard seems to suggest that there is evidence that the parietal region has especial concomitance with consciousness —a tendency to localization which seems to the present writer exceptional and in the long run misleading.

Here as everywhere in such work we are almost blocked from continuing upward and outward into the cortex by our black ignorance as to the nature of the neuronal energy, neurility. Still, here and there a step ahead is taken. For example the all-or-none principle (first applied to hearts, then by Lucas to voluntary muscle), need apparently be thought of as a law of action of the neurones also; any stimulus which actuates a neurone at all actuates it to its physiologic limit and in all its parts. This principle, if real for the nervous system, brings us willy nilly back again to the old familiar but still elusive *synapse*—the all-powerful but mythical genius which every rubber of the lamp may command at will. The process of central association concomitant to euphoria, however, remains unguessed, for the interpretation of the synapse-concept in structural terms of the material neurones is as various as its interpreters. None the less, work along the lines travelled by Sherrington, James, M. Meyer, Thorndike, Parmelee, Hollingworth, and the rest can hardly fail to lead us aright, late or soon. At present we seem capable of discussing to advantage only some phases of the afferent circuits concerned —and these, as is obvious, only with uncertain and halting tongue. This then is one opinion: that there are definite affective innervations not yet mapped out, because of the complexities and our crude and ancient methods, tied to tradition.

Another opinion has already been hinted at in our introductory sentences: It may be idle to seek viatility in Morat's sense of this useful term simply because, so far as euphoria and

dysphoria are concerned, there isn't any. Viatility, the determination of separate routes, may be confined to the "final common paths" of the attention-line (10) whether motor or ideomotor. From this position euphoria is determined (omen absit!) by an abundance of neurokinensis entering the gray fabric and by its flooding in normal amplitude and perhaps in every part the central nervous system, filling it full of happy energy.

III. Summary

1. The basal feeling-tones (euphoria and dysphoria) so far as physiological, are more or less "determined" by the environment of the receptors, euphoria representing the personal reaction when more or less perfectly adapted to its environment.

2. Three factors not yet adequately considered, contributory to the euphoric cenesthesia, appear: *A*, nutritional and sympathetic influences from the active intestinal villi; *B*, kinesthesia proper; and, *C*, the epicritic (dermal) impulses.

3. The 4,000,000 villi of the intestine, rich in smooth muscle and sympathetic nerves as well as in epithelium, probably adapt the blood's content of the nutritive lipoids and proteins to the varying immediate needs of the nerve-cells, and may besides send inward sympathetic influences which, fusing, possibly in the brain, become euphoric.

4. The tonus and the active contraction of the voluntary musculature (by way of the articular, muscular, tendinous, osseous, and dermal receptive fields) make variable but essential contributions to the dynamic "reservoir" of the central nervous system.

Moreover (Bergson) kinesthesia probably adds much of euphoric trend to both the intensity- and the extensity-aspects of all the senses.

5. The integrated epicritic impulses (from the skin and mucosæ) appear to have predominance in human physiologic euphoria, two possible means of stimulation being evaporation and oxidation.

A list of the more or less influential elements of the skin would include the complex vasomotor mechanism; the arrec-

tores pilorum muscles; afferent sympathetic dendrites, the peculiarly efficient epidermis, sweat glands, sebacious glands; Meissner's corpuscles, the terminal cylinders of Ruffini, tactile menisci, the nerve-rings of Bonnet (?), Vater-Pacinian corpuscles, free nerve-endings, heat-receptors, cold receptors, pain-receptors, pleasure-receptors, and possibly tickle-receptors.

6. Air that is dead (*i. e.*, not moving); humid and too warm; humid and too cold; or lacking in oxygen, is a chief occasion of organic dysphoria. Physiologically these conditions are lacks, —lack of movement over the skin, lack of the optimum temperature, lack of dryness (evaporation so being lessened), and lack of dermal oxygen, possible reflex determinant in part of pulmonary respiration.

As regards neural dynamics, these lacks may be deemed productive of deficiencies in the cenesthetic streams (or stresses, strains, and shears) which support the cerebral neurokinesis ("reservoir")—absence of normal stimulation from the environment.

7. Adopting for the nervous system, as we must, the all-or-none principle, the actual neurology of the euphoric and sthenic balance becomes an interpretation of the synaptic relations in the body's action-system; or, better, an idea of inundation of the central nervous system by euphoric energy.

8. Physiologic euphoria is, then, more or less determined by ample and unimpeded and undeflected neurokinensis flooding the neural gray from the kinesthetic receptors and from whichever dermal receptors represent the influence of the moving air of optimum temperature over the body whenever the cerebral neurones are not short of their lipoidal protein food.

This unimpeded flood of ample neurokinesis is the condition for a high sthenia capable of actuating or inhibiting vigorously a rapid succession of motor paths.

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A PICTORIAL COMPLETION TEST[1]

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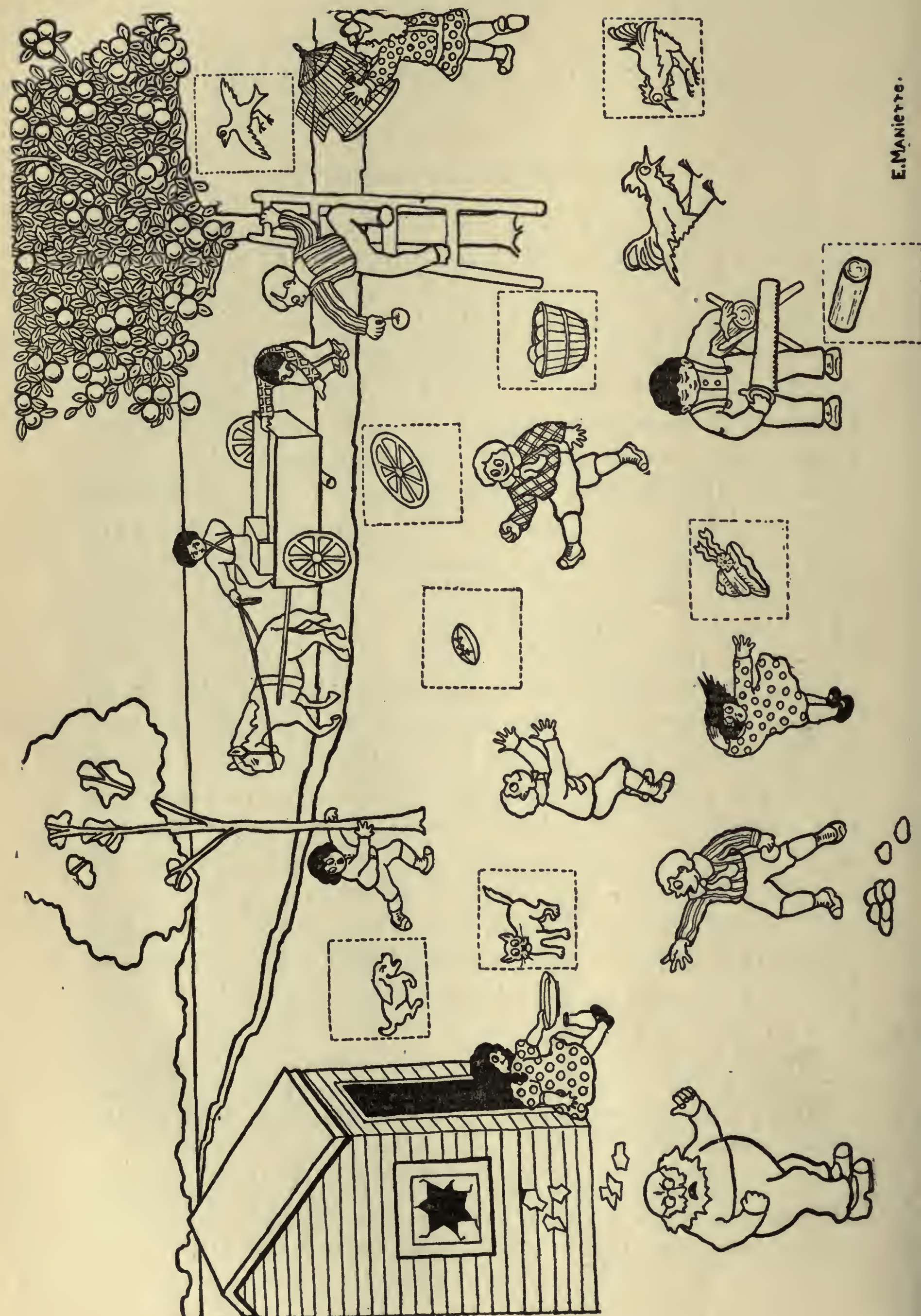
The presentation of this test has much more general and more important bearings than the establishment of its validity and norms. Several main principles for the interpretation of mental tests in general are involved.

On account of its fundamental relation to general understanding and to intelligent control of behavior, apperceptive ability is of vast importance. The wide recognition of the *Combinationsmethode* test of Ebbinghaus shows the general interest in the idea of estimating the power to make connection between different portions of the mental content. We have long realized the significant part which a measure of general apperceptive ability might play in the development of such a psychogram as we desire for our practical, clinical ends as students of the psychological factors of misconduct. But on account of the difficulties in comparing justly individuals of many grades of ability and experience in handling our own and other languages, we have been obliged to reject almost totally the Ebbinghaus method of filling in the vacant spaces of a visually presented text. The idea came to me that one might measure to some degree reagents' apperceptive abilities by asking them to fill in blank spaces where parts of the meaning of a picture had been left out. After considerable experiment a form of picture test was evolved which has been in use now for over two years, and which has received favorable comment from various users.[2]

[1] This test was first reported at the twenty-first annual meeting of the American Psychological Association. It is to be regarded as an addition to the "Tests for Practical Mental Classification" described in Psychological Monograph No. 54, issued by the Psychological Review Publishing Co., March, 1911.

[2] The gratitude of all users of this test is due the artist, Mrs. Eleanor Manierre, who through intelligent appreciation of the conditions, and by virtue of her experience in illustrating for children, was able to turn out a picture which time proves is so little open to criticism. The Wallace Press, of Chicago, showed much interest and generosity in carefully working up the lithograph.


E.MANIETTO

The brightly colored picture, 10 × 14 inches, represents an outdoor scene with ten discrete, simple activities going on. When properly mounted (best on 3-ply scroll-saw wood), 10 one-inch squares are cut out along the dotted lines shown on the lithograph, each square bearing upon it an object from the picture necessary to complete the meaning of the separate activity. Besides these ten pieces, there are 40 other one-inch squares (produced as part of the original lithograph, but not shown in the illustration), 30 of them bearing other objects, and 10 being blank. Each piece should be so well cut out that it may fit into any one of the ten apertures; there must be no possible rejection of a choice on account of variation in form or size.

The background of the picture in general, and of each piece, with exception of the window, is all the same solid color. This involved considerable difficulty in the drawing, for it meant the obliteration of most of the perspective, and making all ideas subordinate to this essential need. Otherwise some clew other than bare meaningfulness would have indicated the proper position of the piece. We have more recently seen the wisdom of working with only 4 or 5 blanks, because these are almost never used. The picture is presented, then, with 10 apertures representing 10 places where the sense is incomplete, and with a stock of 45 ideas, as it were, to draw upon to complete the sense. The analogy to the Ebbinghaus method of completing sense in a verbal passage may at once be seen.

This test has desirable simplicity for presentation and for interpretation of results. The task is readily understood, and enlists the interest of all. The development of a *method of presentation* was not difficult. It seems most satisfactory to begin by explaining that any of the pieces will fit in any of the spaces, and that the surroundings of each aperture should be studied carefully to see what is needed to complete the sense of the picture. In order to be sure the task is understood well, one activity is taken as an example. The examiner points to the wagon group and says, "What is missing from here? What is the man looking for?" Everybody says of this, probably the easiest space to fill, "The wheel is gone." The

reagent finds and puts in the wheel, and is told to study and complete each group activity in like manner. Rapidity is suggested only at first, when it is mentioned that the task should be done rapidly as well as correctly.

The *scoring* is most simple. Each aperture is recorded by name as it is attacked, and the piece placed is also given by name. If changes are made, the previous name is crossed out and another added. Thus:

Wheel...........................	wheel.
Dog.............................	(mouse) (stool) dog.
Hat.............................	(purse) hat.
Log.............................	log.

After all the spaces are filled, the reagent is asked if he is quite satisfied with his placing of the pieces, and, if not, is urged to correct them. Time is taken when the reagent says he has finished the task and made all the corrections he desires.

Although logically out of order, we must here, for the understanding of what follows, state that errors are obviously of two kinds, rational and irrational. From a common sense view of the different interpretations of the activities, and from the demonstrated apperceptive powers of children in all groups, one can decide with minimum arbitrariness which are the few errors to be denominated as logical. The criterion is readily gained by asking for introspection. If many reagents think that the unbroken window accords well with the meaning of the window group, or that a base ball is not necessarily incongruous in the place intended for a foot ball—both arrangements showing neglect of only smaller features in the given situation—then they are not errors of the magnitude involved when the whole meaning is missed. On this basis (the common sense features of which make it unnecessary to burden the reader with the long tables of specific errors which we compiled) we find the following 10 errors to be the only ones which either are common, or are explicable by the reagent in the light of almost full appreciation of the situation.

Broken window—unbroken window. (The commonest error, but one made through not perceiving the significance of the angry man, the boy with stones, and the falling glass.

Unfortunately, some people fail to find the latter well enough illustrated.)

Foot ball—base ball. (Also a common error, but clearly one of carelessness, and readily discerned by even some of the feebleminded.)

Flying bird—standing bird, or blank. (The first is not seldom given as satisfactory, although search for a better idea easily reveals the error. The second is rare, but, of course, is quite logical.)

Chicken—mouse, or cat. (The chicken might be jumping at either of the last two. Seldom.)

Cat—cat walking away. (Of course cats are not supposed to be walking away from milk, but perhaps the girl is persuading it. Seldom.)

Hat—purse. (The wind possibly might have blown away the girl's purse. Unusual.)

Log—axe. (The axe so obviously might belong to the scene that the position of the saw is neglected. Unusual.)

Basket—baby. (Very rarely given, but when the man's action is explained as bringing down the apple to comfort the baby, certainly it is quite logical, even though unhygienic.)

But even the above errors are all avoided by naïve appreciation of the details, as will be shown in a minute. Hence we know that they should be reckoned as errors, although not of the worst class. Indeed, there is not one of the above situations, apparently logical to some reagents, but which has its fallacies detected by some high-grade feebleminded person. I suppose that with thousands of observations one could work out a numerical evaluation of the egregiousness of the different errors, but such a schedule might be logically dangerous in its application to the single case.

Norms.—In the discussion of norms I shall proceed from the performances of seven groups of individuals; (*a*) unusually bright young children; (*b*) 110 children from five grades in a well-known private school, as recorded by Miss Clara Schmitt during the progress of other testing there; (*c*) 248 cases—nearly all of them repeated offenders—seen as delinquents in our Juvenile Court work when a full round of tests was given

and careful diagnosis of mental ability or aberration made; (*d*) 15 cases selected for me at Vineland as representing the brightest feebleminded individuals there; (*e*) 95 college people, the general record of whose errors has been most kindly forwarded to me by Professor Eleanor A. McC. Gamble of Wellesley; (*f*) unselected intelligent adults; (*g*) cases of psychosis. From these we derive the following results that clearly have practical bearing upon use of the test for diagnosis.

(*a*) Young children of more than ordinary brightness do this test very well. Taking the nearest birthday as the age, as we do all through, Miss Schmitt found three children of 8 years old in the private school doing it with no errors at all—two of them in less than 4 minutes—and two more with no illogical errors. Others of us have made like observations. Anyone can get in this way most convincing proof of the validity of the test for naïve minds. Take an example of a slow-going and thoughtful little boy seen by me; 8 years of age, by Binet $10\frac{4}{5}$. He took 7′ 35″, made only three changes, studied out each situation carefully, in the meantime revealing his mental processes as follows: "I think there is something wrong there, the window is broken." "Somebody or something is chasing that boy." "Something is blowing her hair." "That cat's eyes are looking that way; I guess that would do." He left the baseball in place as his only error, but was not familiar with footballs.

No doubt there are precocious children who would give nearly a perfect record at a younger age if they have had sufficient experience with the world. We have made no attempt to find such cases.

(*b*) The following tabulation of the school group speaks for itself.

(*c*) The selected material offered from our Institute, homogeneous in the respect that all are offenders, is necessarily handled in a different way. School grades alone mean very little. We have classified according to an estimation of ability as judged by a wide range of tests, performance on school work, etc. We still follow the practical classification given with our first published tests in Psychological Monograph No. 54. It

should be understood that the grouping is only for this general run of cases seen in juvenile court work. Class *A* would undoubtedly be supernormal, even in the private school group, but probably the ordinary range of ability would be higher in such a school.

Age at Nearest Birthday.	Number of Cases	Grade	Limits of Total Errors.	Limits of Illogical Errors.	Median Total Errors.	Median Illogical Errors.	Range of Time.
8	12	2d	0–7	0–7	2	1	3′–9′
	2	3d	0–3	0–1	1½	½	
9	5	2d	0–3	0–3	2	1	1′–9′
	13	3d	0–4	0–3	1	0	
	4	4th	0–4	0–2	1½	½	
10	6	3d	0–3	0–2	½	0	2′–6′
	14	4th	0–3	0–2	1	0	
	3	5th	0–3	0–3	1	0	
11	4	4th	0–1	0–0	1	0	2′–6′
	7	5th	0–3	0–1	2	1	
12	2	4th	0–2	0–0	1	0	2′–7′
	7	5th	0–1	0–1	0	0	
	13	6th	0–3	0–1	1	0	
13	4	5th	1–3	1–2	2	1	2′–7′
	13	6th	0–6	0–4	1	0	
14	1	5th	1	0	1	0	3′
	110						

Class *A*, viz., distinctly above the ordinary in ability and information. 5 cases, ages 8 to 17. Of these only 3 made 1 total error each. The only illogical error was by the youngest, 7 yrs. 9 mos. (Binet age through 10 yrs.) Time range 3′ 17″ to 5′ 30″.

Class *B*, viz., ordinary in ability and information. 44 cases. Ages 9 to 17.

Median total error, 1. Range total errors, 0–5.
Median illogical errors, 0. Range illogical errors, 0–5.
Time: range 1′ 20″ to 6′ 50″, median about 3′ 30″.

Only two made more than 1 illogical error; the single 8-year-old made 3, and one peculiar adolescent, not however determinably aberrational, who did well on other tests, made 5 illogical mistakes. We have in fairness to include this rare

case, but probably it should be explained by some emotional upset, for which we try to be on the lookout. Individuals of 10 or 11 years are, naturally, comparatively infrequently seen as offenders. The four such cases we did study, belonging to the *B* group, made no errors at all.

Classes *C*, *D*, *E*, *F*, may, for our present purposes, all be grouped together as being what we have denominated as fair in ability, but for various reasons not grading as high as class *B*. 72 cases. Ages 9 to 18.

Median total errors, 2. Range total errors, 0–5.
Median illogical errors, 1. Range illogical errors, 0–3.
Time: range 1′ 30″ to 11′ 8″, median about 3′ 30″.

Classes *G*, *H*—both graded as poor in ability. 26 cases. Ages 10 to 17.

Median total errors, 2. Range total errors, 0–5.
Median illogical errors, 1. Range illogical errors, 0–4.
Time: range 1′ 35″ to 9′, median about 4′.

Classes *I* and *J*. Dull from physical causes, or subnormal mentally, but hardly to be denominated feebleminded. 31 cases. Ages 10 to 18.

Median total errors, 3. Range total errors, 0–7.
Median illogical errors, 2. Range illogical errors, 0–7.
Time: range 2′ 13″ to 12′ 47″, median about 4′ 30″.

Class *K*. Feebleminded—moron grade. (No imbeciles were tested.) 37 cases. Ages 10 to 18.

Median total errors, 5. Range total errors, 1–8.
Median illogical errors, 4. Range illogical errors, 0–7.
Time: range 1′ 45″ to 10′ 4″, median about 5′.

Two individuals each left one total, but that an illogical error. They were 12 yrs. and 15 yrs. old, and by Binet proved 9 and 8⅘ years respectively. Two others each left 2 total, but no illogical errors. They were 11 and 17 years old, and by Binet each proved 9 years old.

Class *M*. Psychoses. 33 cases. Of course this pathological group is not homogeneous, and figures on a norm for it are worth nothing. Results ranged all the way from no errors at all to 8 illogical errors, or to leaving no pieces in place. Of this later.

(*d*) At Vineland Superintendent Johnstone called in those who seemed to him and other officers to be in general the brightest members of their colony. 15 cases. Ages 16 to 33.

Median total errors, 5.	Range total errors, 1–7.
Median illogical errors, 4.	Range illogical errors, 0–7.

Time: range 3′ 15″ to 16′, median about 6′.

The best two records are as follows: A young man, considered the most intelligent male at the institution, did the test in 16′. He studied each place with very great care, made only two changes, and finally left merely one error, viz., the baseball for the football. He corrected knowingly the unbroken window. This boy was 21 years old, Binet age 10. Better still was the record of a girl, aged 16, Binet age 11. She finished in 3′ 30″, made 3 changes, including replacing the baseball by the football, but left the unbroken window as a final error.

(*e*) The Wellesley group of 95 college people shows:

Median total errors, 2.	Range total errors, 0–8.

Time not calculated on the same basis as in our cases. The logical errors are not given for each case, so we can give neither median nor range, but by introducing other comparisons valuable results are obtainable.

Cases with no errors:

Wellesley	26%
Private school	30%
Delinquents, *B* group	33%
Delinquents, *A* group	40%

Percentage of total errors to pieces placed:

Wellesley	21.7%
Private school	15%
Delinquents, *B* group	10%
Delinquents, *A* group	6%

Percentage of total errors illogical:

Wellesley	64%
Private school	50%
Delinquents, *B* group	40%
Delinquents, *A* group	33%

Comment on these interesting comparisons will be made further on.

(*f*) Unselected intelligent adults. Very slight experience will show that many intelligent adults when confronted by simple mental tests show curious and unexpected reactions. They frequently do much poorer at them than children. All of us have noted this point and spoken frequently of it. A typical case would be the woman who looked over the completion picture critically, and could find no more reason for putting the hat in place than the purse, or the cat than the baby ("That girl appears to be foolish; she may be teasing the baby with the milk"), and so left both couples of pieces at the side of the empty squares. The difference was also shown when a professor of psychology left in the chicken tail down, a mistake (not counted as an error) which we have never seen a normal child tolerate. The same is true of other performance tests —capable adults becoming quite provoked because they do not do well at them. I myself made one of the worst records ever made in the mirror tracing test at a certain laboratory.

(*g*) Psychoses. The results on this test as performed by individuals with psychoses are sometimes most interesting; in other cases not at all so. Of course, some types of insane people may do such a test most brilliantly. Our attention was drawn to its possible value in this connection by the performance of a young woman whom we left for some moments, putting the test before her by way of occupation. After some time we noted that she had no piece in place. We went over the method with her again. At the end of one hour she had no single piece in place. Every piece she had placed more than once correctly, showing her good apperceptive powers, but she acted as if she could not be sure that they were correct and so would take them out again. It proved to be a marked case of *folie du doute*. The curiously slow and dreamy performance in some cases of dementia precox, and the accelerated, but erratic perceptions of some maniacal conditions, are also characteristic. We have had no means of doing more on this subject than observing bare possibilities of usefulness. From other places where the test is used, I hope there may be forthcoming a statement of findings on numbers of cases of various types.

Argument

In the above findings we perceive important correlations and differences. The performances of supernormal young children, whether as seen in our court work or outside, tally well with each other. The record of our *B* group compares closely and favorably with the private school children. The former ranges better, perhaps because some individuals of poor ability (as would be so diagnosed by the round of tests which we use in our institute work) figure in the school totals. We particularly note that individuals 10 years of age, whose demonstrated ability is good, do as well on the test as older persons of the same general mental rank.

The feebleminded group, as seen under two conditions, tally well in their extremes only. Nearly all make bad failures. It is obvious that rare individuals among them have developed the ability to apperceive such relationships as are demanded by this test, even though on other levels their mental equipment is demonstrably poor. This is as we, who observe the special abilities and disabilities of these mental defectives, as well as of normal persons, would expect. It is not to be anticipated that any single test can be evolved which will discriminate the feebleminded.

Then there are all degrees of feeblemindedness, and many of moronity, hence working up norms on a single test is almost valueless for establishing characteristics of such a widely varying group as may be scheduled morons, to say nothing about variance in special abilities. I am still looking forward to valuable psychological work being done in the study of such variations.

In the errors of the feebleminded it is often a matter of no little interest to ascertain just what the association of ideas was which led to placing of the given pieces. An example was the soliloquy of a high grade moron when he placed a clock in the field by the girl where the hat belongs and said, "That goes there. She's scared because she's late to school." Evidently her gesture and dishevelled hair were to him indicative of fright; fright might be induced by tardiness at school; the clock was a symbol of time and tardiness.

Individuals with psychoses may show very curious and characteristic performances. This is, too, as we should expect, since, after all, the criterion of insanity is behavior. Such a test which, much more clearly than the automatisms of ordinary conduct, shows the mind at work, might well bring out peculiarities of mental action.

When we come to the differences between the results of intelligent adults and of younger people, the former not only showing no increased facility of performance, but even a set of worse records, we are in the face of a phenomenon vitally important for all clinical psychologists. We have had, as mentioned above, intimations before this that the cultivated adult type of mind does not produce good scores with various tests which are satisfactorily used for younger individuals. But the records of Professor Gamble demonstrate this as nothing else has done. What is the reason for the difference? At present we have all too little knowledge of how age and cultivation affects mental performance, but one may be allowed to speculate a little on the causes of difference—especially since observation of comparative attitudes and the hearing of the adventitious remarks during performances leads us to know something of what is going on in reagents' minds.

In the first place let us say that we have frequently remarked in our work with delinquents, in testing and in other connections, the changes in attitude which characterize the passing of naïvete and the acquirement of mental sophistication. The older individual is prone to meet a simple situation with the idea that there must be something deeper back of it. We have seen this even in the presentation of our construction tests to some men of marked ability.

Or the older person assumes a critical attitude that the younger does not think of taking. This picture with its absence of perspective and its lines on the order of caricature, such as children themselves produce, was made to appeal to the simplest minds, and perhaps that creates for it a limitation. The "might be" of the adult, with his greater stock of ideas, is very rarely heard from the child, unless there is unfamiliarity with the represented situation. A marked example of this is

the chicken insert. Of course the chicken "might be" jumping at the cat, or at the bird in the cage. The greater experience of adults led them to perceive many more possibilities in the situation than the child sees. It may be this, rather than any conscious attempt at criticism, which leads the adult to go much farther than taking the picture at its barest face values. Again this difference may be observed in other tests. When asked for the Binet discrimination between a butterfly and a fly, what adult would be satisfied with answering, "One is yellow and the other is black"—a response quite sufficient for passing the test.

Then simple tests are frequently approached by the adult with an air of jocularity which may interfere both with doing rapid and careful work. Lack of ingenuousness here again is a barrier to good results.

The differences between our records and the Wellesley findings can hardly be due to difference in method of presentation, because the test is very simple, and all groups were given ample chance to correct their errors. Whether or not the college people really tried to do their best, as children do, I cannot answer. That is a question which only rarely has to be met in working with the younger groups.

It stands out clearly that if certain other less direct methods of scoring were used, as they might be in many tests, adult experience might rank high. For example, if we were to ask for all the subtle possibilities of the situation expressed by the girl with milk, in relation to the stock of ideas on the unplaced squares, one would immediately get a variety of responses from an adult which would be quite beyond the ken of children. Thus maturity would be vindicated.

As a test for mental age this completion picture seems to have as much substantial validity as most others, with this addition that it is a real test of ability in itself, for it is done very little better in after years than when first the ability is developed. From the tables of the private school group we learn that at 10 years the performance is as good as it is at 13 years. In fact, at 9 years the results are not far behind. Back of that we get a large number of bad failures. It was so

evident from the start that younger children as a rule failed that we have never worked up these negative findings. Professor Gamble sends us the general record of 15 children, average age, 8 plus. This shows that 66 per cent. of all final placements were errors, and that 87 per cent. of these were illogical. So far as time is concerned this is not markedly decreased after 10 years of age.

On account of the findings in the college group it is important to note that we observed no decrease in ability in our *B* group up to 17 and 18 years.

The time element, except when very rapid or very slow, seems to mean little. It is easily perceived that one person may deliberately adopt a slow or quick method, unless rapidity is emphasized as much as it is in other tests—and that would be undesirable here.

All through, we see illustration of the fact that interpretation of the results on mental tests has to be made by the use of wide limits, and with due regard for the different norms which exist for different ages—average records even possibly becoming poorer with adult life—and for the experience of different social groups. In testing those abilities which involve intelligence, interpretation along anything of the fine lines laid down for the use of laboratory psychophysical apparatus would lead to unjust estimation of personal capacities.

The idea of this completion method apparently is valid, but our picture may not be at all the best that can be devised for establishing norms of apperceptive powers. For older persons and for other groups of subjects a different picture or set of pictures may be worked out with more difficult or easier tasks involved, as in the Ebbinghaus texts. Perhaps the idea may prove valuable in several directions, as have our previously published performance tests, for instance, in the immigration service.

Summary

We evidently have in our completion picture a test for ability primarily adapted to the child type of mind. Every detail of the meaning has proved to be understandable even by morons. The performance of naïve individuals of ordinarily

good intelligence above 10 years of age should be better than in 5′, and not more than one "illogical" and two total errors should be made. A worse record than this should arouse suspicion of defect in mental ability.

We cannot expect a much better average performance with increasing years; indeed, such good results may not be obtained from older cultivated individuals. The use of the test has established a vitally important point for clinical psychologists, namely, that performance may be worse, according to simple methods of scoring, when adult experience is brought into play. The carrying over norms from one social or age group to another is not justifiable without actual establishment of validity.

As good a record as that of the standard given above may rarely be made by individuals who are defective according to other tests.

We see no correlation between apperceptive powers, as tested by this completion picture, and recidivism in juvenile delinquents.

The performance gives a remarkably good chance to see the mind at work. Mental control and association processes are peculiarly laid bare, and for the study of these in defectives and aberrational individuals this type of test offers much.

CANNOT PSYCHOLOGY DISPENSE WITH CONSCIOUSNESS?

BY ELIOTT PARK FROST

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In his essay: 'Does consciousness exist?', posthumously published, William James writes: "For twenty years past I have mistrusted consciousness as an entity; for seven or eight years past I have suggested its non-existence to my students. . . . It seems to me that the hour is ripe for it to be openly and universally discarded."

Such a pronouncement is welcomed, *von Anfang an*, by the Realists and Radical Empiricists in philosophy, who accept the term 'consciousness,' as name for a function of the thinking process. Such a pronouncement is equally welcome to the students of animal behavior, who read in it, however, only a confirmation of what they have long proclaimed: the non-essentiality of the concept of consciousness. Strangely enough, it is in psychology itself that a readjustment in extrinsic terms proves difficult. The self-consistent textbook of a psychology, thus objectively viewed, has yet to appear. If one is ever to be written, it can neither ignore what is commonly termed 'conscious process'; nor may it still use the psychic terminology uncorrected.

The business of our discipline is to describe, and to explain, certain phenomena. Description heretofore, has meant to talk in mental terms: of sensations; of percepts as synthesized sensations; of memory, of imagination, and the like. Explanation heretofore has meant to give the physiological correlate of these psychic processes: to speak of neurones, synapses, and final common paths. Whatever nice distinctions the instructor may make for himself, it is certain that the average student leaves his first year in psychology with the notion of two very clear-cut entities: a physical process that one can tap, detect,

measure; and a further somehow-psychic process, part experienced through introspection, part conceived of.

At the outset I show my hand. It is my personal bias, that, as data for a course in elementary psychology at least, psychic terminology with its present connotations, is confusing. I deplore the awkward straddle in which psychology is at present. We are trying to ride two horses at once, the horse of philosophy and the horse of behavior, and they are proving as mutually irreconcilable as the horses in the Platonic figure, one horse being white, and the other horse black. Let us keep to one mount or the other with some consistency.

"Whoever blots out the notion of consciousness from his list of first principles," must indeed as James says, "still provide in some way for that function's being carried on." It is my present, perhaps over-ambitious intention, to attempt an explanation of the "fact that things not only are, but (that) they get reported, are known," in a strictly physiological way, without any conscious or psychic implication whatsoever.

The term 'awareness,' emphasized by James himself, and reimpressed upon students in every psychological class-room, has always a mental, rather than a physiological connotation. To be aware is to be conscious. To prove awareness is to prove the presence of consciousness. Once establish the processes of awareness in an organism, and one demonstrates the presence of consciousness in that organism.

I beg that for a few moments, however, you will agree with me to understand the term 'awareness' in a different sense: namely, that it shall have exclusively a physiological import. So that, if you grant a friendly ear to this proposal, we shall speak of a physiological mechanism as, at times, becoming aware. And we shall test the presence of such awareness on the part of such a mechanism according to the usual criterion of behavior: does the given stimulus elicit from it a response. If, from a nervous structure, a stimulus does elicit any sort of response, in so far this shall sufficiently justify the assertion: "the organism is become aware of that stimulus." If, on the contrary, the nervous structure, upon irritation, makes no response of any nature, it must be then maintained: "the

organism is unaware," or, to use the synonym, is 'insensible.' And we shall remember that all the while the usual mental or psychic connotation of these terms is to be rigidly excluded.

With this distinction as a basis, let us investigate a simple type of behavior.

Within a dark room suddenly flash upon a nervously sensitive visual end-organ, a beam of light. If the pupillary reflex take place, as will transpire in the normal eye, it may then be affirmed that the eye-mechanism has become aware of the stimulus; and the resultant behavior, namely, the closing of the eye, is proof positive that such is the case. Such a primary reflex may occur, of course, wholly independent of any functioning on the part of higher neural centres.

Let us designate such a simple sensori-motor path as that involved in nervous response to a stimulus, as an *Alpha-arc*. The term alpha-arc shall then characterize any simple, single, sensori-motor path, initiated by some peripheral stimulus, and resultant in some end-effect. For an alpha-arc to function it is merely necessary that stimulus elicit nervous response.

But an alpha-arc may involve a longer circuit. If, in the dark room, the tiny flash come now from the side, it will naturally be met by a turning of the eye-ball, to bring the stimulus within the direct line of regard. Beyond structures involved in the previous illustration, there are now included at least the thalami, and the optic lobes. Still, however, we shall speak as before, shall we not, of a sensitive mechanism that has become aware of a stimulus, and has responded to that stimulus by adaptive behavior? We still have an arc, have we not, that may run its course independent of any so-called 'conscious coefficient,' and still result nevertheless, in accommodative behavior? In the lower organisms we call such behavior tropic; in human behavior we speak of it as completely habitualized or instinctive reponse.

As a matter of fact, however, whenever an alpha-arc functions so as to include the specific cortical structures, for instance, the temporal or optic lobes, we presume that there is always the further likelihood that the great complication-areas, or association areas of the cortex, be stimulated as well. When

such a further arc, aroused by an alpha-arc rather than by a peripheral stimulus, is set in function, let us denote it as a *Beta-arc*. In brief, then, when a stimulus falls upon a sensitive neural mechanism, it will normally arouse an alpha-arc, and this alpha-arc *may* in turn arouse a beta-arc; the latter being alone provoked by the alpha-arc, and presumably having for its seat the association areas of the cortex. The only experimental criterion of such a beta-arc will of course, again be behavior; but behavior significantly different from the response of the simple alpha-arc. Just what this modified type of beta-arc response involves can be shown in a moment.

In the case now before us, we speak of the beta-arc as becoming aware of an alpha-arc, in precisely the same sense that we spoke a moment ago of the alpha-arc as aware of the external stimulus, and for the same reason: it responds to stimulation, characteristically. The alpha-arc is unaware of itself, but aware of the external stimulus; the beta-arc is unaware of *itself*, but is aware of the alpha-arc. Only when and if the beta-arc in turn arouse some still subsequent arc, as a gamma-arc, can it itself become an object of awareness.

Once again, then, all simple sensori-motor arcs having their loci in the first and second levels, to use MacDougall's phrasing; aroused by stimuli peripheral to the central nervous system, and eventuating in behavior, we shall call alpha-arcs. And all arcs which are stimulated alone by alpha-arcs, having their neural seat in the association areas, and related only through intermediate structures to the end-organs, but still eventuating in characteristic behavior—these we shall call beta-arcs. Neither an alpha-arc nor a beta-arc is then aware of itself, but the beta-arc becomes aware of the alpha-arc; the alpha-arc becomes aware of the external irritating object. In a word, no arc is self-sensing, but any arc may become object for a succeeding arc. And what holds of an individual arc, holds equally true of complexes or nexuses of such arcs.

By introspection, psychology alleges that one holds for a moment as object of an awareness-process, a just-previous process occurring within one's own private nervous system. But such a prior process is commonly dubbed 'mental,' or

'psychic,' and the assertion is made that while such a process is unaware of itself, it reflects upon another mental process, contiguous, but just prior. Even for the introspectionist, that is, no process can become aware of itself. All introspection involves reflection, temporal succession. For the introspectionist there must be two processes, both alleged psychic in nature, where one process takes for its object another process, just prior.

In accordance with the tenets of ordinary psychology, then, if consciousness were cinematographic, instead of continuous, mental life would be impracticable; for while the integrity of the first process would remain unimpaired, this process could not become aware of itself, nor by hypothesis would it remain as object for a succeeding process, and hence there would exist the possibility of no awareness-experience whatsoever, whether dubbed 'conscious' or physiological. In short, if there were no memory; if no content cast its influence beyond the fleeting instant, and life were but a succession of momentary incandescences, each swallowed up in oblivion so soon as completed, then reflection, and with it introspection, would become out of the question.

Well! If no process can experience itself, be within itself both subject and object, what grounds exist for labelling any such process, 'psychic'? Is not the ascription to such processes of a psychic content, a wholly gratuitous performance? Are not the mutual relations of these arcs to be understood in simpler terms?

This the present writer believes. For him the term 'consciousness' with its psychic implicates, has long seemed to be a misnomer. Within his so-called 'world of experience,' he can find no psychic attachment. The whole conception of a mental, extra-physiological experience appears to him only as a pleasant speculation for the philosopher to play with; but one that becomes useless, if not misleading, for the teaching of psychology as a science. Is not behavior adequately explained as the resultant, in our terminology, either as the functioning of an alpha-, or as the functioning of a beta-arc, or group of such arcs? Does not this cover all that is now meant by 'conscious process'?

If we yield (perhaps to the philosopher), all 'life-rights' in the term 'consciousness,' on the ground that it is a misleading concept, we can then speak of an alpha-arc as an awareness-process, having for its biological goal the immediate adjustment of organism to stimulus. In definition of the further beta-arc process, the present writer has elsewhere suggested the term: 'consciousizing process.' The biological significance of this latter lies in its ability to take as an object (*i. e.*, to be stimulated by), prior awareness-processes, or cortical retentions of such processes (commonly called images), or both. In this way the immediate afferent impulses (alpha-arcs) are influenced by previous organic experience, and resultant behavior is correspondingly modified. In the simple awareness-processes, the nervous impulse has a fatal course to run, passing from the point of stimulation in the end-organ directly to the musculature over hereditary or canalized paths. But if the alpha-impulses in passing, set off further beta-arcs, facilitation or inhibition of one sort or another takes place, and the final behavior is modified, since there is now involved elaboration of the original impulse in terms of the past experience of the particular nervous system implicated.

There is no reason to suppose that nervous impulses differ in kind. An alpha-arc and a beta-arc differ only in that they have unlike objects. In the one case the object is a peripheral stimulus; in the other case it is a central one. If one prefer to call this awareness, on the part of one nerve path, of another nerve path, 'conscious awareness,' to distinguish it from the simple reflex type of awareness, there can be only the objection that the majority of present-day students of our discipline will feel that somehow something psychic has crept in; a conception, from our point of view, both useless and disconcerting. If we can but purify our psychological concepts of their mysterious connotations, there may be advantage in continuing to use the old-time nomenclature.

Alpha-arc awareness is after all unlike beta-arc awareness, *as a neural experience*. In speaking of the latter we might still say: 'sensation;' 'image;' 'thought;' 'mental process.' So far as these everyday terms express only a unique physio-

logical experience, different from a mere awareness experience, there is no excuse for replacing them by neologisms. But sensations will no longer now be 'first things in the way of consciousness,' but the second. There must always be at least two physiological processes, successive in time, for us with propriety to call one of them a consciousizing process, or sensation. The iris, responding to a light stimulus, can never get a sensation.

At just what point in biological evolution, beta-arcs first evolved to supplement the simple alpha-arcs, is no doubt largely a matter of speculation. They would seem to be called for, however, when the reflexes prove no longer adequate to cope with the environment, except by reference to past experience.

Past experience, retained as cortical disposition, may of course become the object of beta-arc awareness, no less than a fresh afferent path; so that the responses sent to the final common paths, to use Sherrington's terms, are the resultants of alpha-arcs, recent and remote, plus the effect of consciousizing processes upon them both.

If we are to speak of a beta-arc in function, as a 'consciousizing process,' we may speak of an alpha-arc as a 'preconsciousizing process,' or at other times, as a 'consciousized process.' The former term would characterize those alpha-arcs that are independent of beta-arcs, being originally autonomous, viz., the reflexes. And the latter term would characterize those alpha-arcs which no longer arouse beta-arcs, although they formerly may have done so, viz., habitual mechanisms. These exhibit the effects of beta-arc action, but no longer normally engender it.

Activity of an alpha-arc gives the organism, say, red-awareness. This means that the retina has responded to an ether vibration of 440 $\mu\mu$ in specific fashion: a different response from that which it makes to vibrations of, say, 790 $\mu\mu$. If this arc now an instant later arouses a consciousizing process (beta-arc) we get what we may call the 'sensation red.' Can either introspection or logic demand any further description or explanation of this 'sensation-red-experience' than to say that a

nervous impulse has passed through the cortex, and there aroused a second impulse which takes it as an object of awareness? Does the postulation of any psychic entity clarify or add to the data thus offered by physiology?

The 'intimate warmth' of which James speaks, does not present us with a Self. The 'warmth,' indeed, is explicable: the object of regard in such cases is a fresh neural path, just now in function. But to become aware of the warmth requires still a separate, subsequent, neural path, as yet unaware of itself.

So ingrained is the 'common-sense view' that physiological processes are the *vehicle* only of psychic processes which play upon and through them, that it is difficult at first to comprehend how a physiological process in function *is itself* consciousness at the moment, completely described; and that any further implication is unwarranted. But cannot psychology approach nearer to the scientific ideal of all disciplines by considering the terms 'consciousness' and 'mind' merely as handy ways for expressing the reactions or awarenesses on the part of processes beta, gamma, delta, and so on; of processes alpha, beta, and gamma, respectively just prior? Is not what is commonly meant by 'consciousness,' so far as psychology is concerned, more nearly described as a 'beta-arc functioning,' than to speak, either of 'an elementary psychic process,' or even of 'a knowing function'?

THE MECHANISM OF MENTAL PROCESSES AS REVEALED IN RECKONING

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Much laborious research has been undertaken in order to throw more light on the reproduction associations. Some psychologists have examined mainly remote memory; others, immediate memory. The reproduction of nonsense syllables, letters of the alphabet, words, numbers, etc., has been elaborately tested, by Ebbinghaus, Müller and Schumann, Müller and Pilzecker, Meringer and Mayer, Wohlgemuth, and the American School of Psychologists. In conditions in which clinicians desire to test the nature and the integrity of the reproduction associations, it is not always possible to utilize the methods which psychologists have found valuable; and even when such methods are practicable, there is no standard by which to measure the results they yield. Of the various methods, that which deals with the reproduction associations of stimulus words has been most used by psychiatrists. From the nature of the response and from the time taken to utter it, deductions have been drawn regarding associative complexes in health and disease. But words are uncertain quantities. The result in word associations is seldom inevitable. We instruct and exhort, but we cannot compel investigated persons to utter the first word, to name the first thing, which rises to consciousness after they receive the stimulus word. This first word may, therefore, elude us. Several persons, even the same person at different times, may give different responses to the same word. Davis and Rosanoff have shown that these different responses are sometimes finite in number; that a given stimulus word, in normal people, may excite the reproduction of a member of a small group of words which are its common reactions. But the feeling tone of a word varies in

different persons and even in the same person, according to mood. We cannot, therefore, place any absolute value upon particular reproduction associations of words. We cannot use anomalies of word reproduction as guides to the relative mental states of different persons. We cannot even use such anomalies as an index to variation in the mental state of an individual. Word associations may give clues to associative complexes, they do not afford a measure of the mental peculiarities of persons and are of little diagnostic value. Word associations reveal little regarding the relative importance of perception, of the psychic reinforcement of sensations, and of the affective element of sensations, in the dominance of attention.

Figure associations are fixed by the mordant action of years of use. The memory of a figure, such as 5, is compounded of an impression of a certain shape upon the visual cortex, of a certain sound upon the auditory cortex, of the idea of "fiveness," of a certain motor arrangement in the speech area, and of a certain motor arrangement in the area of the forearm and fingers. The resultant of these visual, psychic, auditory, and kinesthetic images forms our mental picture of five, a picture which from infancy to old age, in the ignorant and in the educated, varies inconsiderably. Special attributes may be associated with certain numbers in the minds of certain people; 7 may be lucky, 13 may be unlucky, 3 may connote the Trinity, and so forth. But the feeling tone of a number is negligible compared with that of a word, and is too significant to be affected by moods. Figure associations are thus more or less fixed and without feeling tone; they have a desirable simplicity; they can be tested in the young, in the old, in the normal, and in the abnormal; for the investigations of mental processes, they have, therefore, in many respects, a greater value than word associations.

When a person sees a printed numeral, for example, 5, and copies it, the seeing and the writing are undeniably related. As the 5 written is identical with the 5 seen, the digit seen is the source of the digit written, and the digit written is the effect of the digit seen.

When a person sees together two different digits, say, 5 and

3, he may write neither, but a third, such as 8. This written digit bears to the pair seen a relation which experience accepts as linking the three digits; thus, 8 is related to 5 and 3 as a sum is to its summands. 8 expresses the result of adding 5 to 3; the digits seen (5 and 3) are again the source of the digit written (8); the digit written (8) is again the effect of the digits seen (5 and 3). To the intellectual processes involved in a simple act of reckoning of this kind, we have, therefore, two guides—the excitant, the digits seen; and its externalized result, the digit written.

If the figure associations are adjusted, tuned, or set for a definite end; if simple addition, subtraction, multiplication, or division be decreed; the digit written is the sum, the difference, the product, or the quotient, respectively, of the digits seen; hence, the same excitant can produce several results. We can deliberately adjust the linking of digits so that the digits seen constantly evoke the effect of one relation. We can so arrange that the only figure relation to yield an externalized result shall be, for example, the relation of addition; in other words, that the figure associations shall be linked so that the digits seen shall excite the writing only of their sums; that the figure associations shall, indeed, be *wholly constrained.* If the associations of the digits seen be wholly constrained, so that the digits written should be, say, sums only, and if, then, a result which is not conformable with this existing intent for addition, a result which is not the sum of the concerned digits, arise, another possible associative reaction of these digits in the presence of this adjustment or set for addition, is revealed. When this aberrant reaction occurs, the digits seen have not been properly guided towards their intended goal; the existing intent or "set" for addition has been temporarily ineffective; what we call attention must, for the moment, have failed in its directive facilitation of the association, since an alien associative reaction or error has arisen.

If, at various times, from the same pair of digits, while the associations are wholly constrained, different errors arise, these several errors represent the externalization, under comparable circumstances, of just so many different possible associative

reactions of this pair of digits. I have endeavored, by studying these irregular associative reactions or errors, to ascertain what happens to the digits which act as visual excitants, and what determines the errors written in reckoning; in other words, I have utilized errors as guides in exploring the mental processes of reckoning.

Our knowledge of errors in adding, we owe to Amberg and Kraepelin and Rivers. They distinguished 'writing errors' —due mainly to increased psychomotor excitability, causing false figures to be unconsciously begun or even completed; and 'thought' errors—due to false associations. Errors were also classified as 'corrected' and 'uncorrected.' Kraepelin and Rivers calculated that of sixty-eight uncorrected and four hundred and three corrected errors they analyzed, 12.5 per cent. were 'thought' errors; 55.4 per cent. were 'writing' errors; and 32.1 per cent. were indeterminable. The most frequent uncorrected error, they found, was the subtraction of a digit instead of its addition; this occurred in 21.32 per cent. of their uncorrected and in 10.92 per cent. of their corrected mistakes. The mere copying of one of the two summands instead of the writing of their sum was met with in 12.5 per cent. of uncorrected, 26.8 per cent. of corrected; no marked tendency to select either the first or the second summands in this error was found. Instead of the proper sum, the sum of the two next digits was found in 10.30 per cent. of the uncorrected errors and in 8.19 per cent. of the corrected. The last sum was added to the following summand in 7.35 per cent. of the uncorrected and in 5.96 per cent. of the corrected. The addition of the newly written sum to the last used sum and sundry other errors, accounted for the remainder which could be interpreted.

I have continued the work they have begun, but have sought to find the significance of the errors; to identify the nature of the errors—anticipations, perseverations, suppressions, etc. My purpose in this was to find not only further information regarding the factors controlling attention, but also to seek an absolute measure of the frequency of associative peculiarities which would be of diagnostic value in abnormal mental states.

Reckoning Test Used.—The reckoning test I used is a modification of that devised by Oehren, and so brilliantly utilized by Kraepelin and his school. It consists of a book, resembling the familiar school copy book, every page of which contains digits, arranged in parallel vertical columns. The space between every two successive digits in a column is equal; between adjacent columns a uniform interval exists; and the margins are of adequate width. Every page has ten columns, each of twenty digits. The changes which I made in the arrangement of the text rendered the ease of writing as constant as possible throughout the page and enabled the number of pairs added to be readily ascertained.

To economize space and gently to complicate the task, the digits are added in pairs *continuously; i. e.*, after a pair of digits has yielded a sum, the second digit of the pair is linked with the succeeding figure in the column, to form a new pair. Each digit (excepting the first in the first column) is thus used twice, for the last digit of one column is paired with the first of the next. Twenty sums are therefore obtained from every column of twenty digits.

Method of Testing.—The test is conducted as follows: The person to be tested is instructed in the method of *continuous* addition. A few seconds' warning is given: then the signal to start. The person adds the digits in pairs as rapidly as he can. At the end of each minute the examining physician calls "Stroke"; the person at once makes a mark under the last sum he has written and proceeds immediately with the addition. By counting the sums between the marks, one can ascertain how many acts of addition, how many units of mental work are done in each minute. The test continues for fifteen minutes.

Key to Test.—I prepared a key printed like the reckoning test, but giving the sums only, in order to facilitate the detection of errors. Every column of twenty sums is printed on a separate slip; the slips are arranged so that the figure columns are vertical and are bound so that the resulting book opens upward not outward; a soft cotton band is interposed between the top of every slip and the binding, so that a slip when

turned up, falls over and lies flat. In number, order, and spacing, the figures on each page of slips tally with the written sums which result from the *continuous* addition of the summands on the corresponding page of the reckoning test. Between each page of slips a blank sheet of paper is inserted to prevent confusion arising from the intermixing of the slips. This modified reckoning test and key were published for me in May, 1911, by Mr. James Mackenzie, Edinburgh, Scotland.

Material Investigated.—I have examined with this modified reckoning test sixty-five persons. Every test lasted fifteen minutes. Fifty-nine persons were examined daily for five to nineteen days each; and the remaining three were examined on two, three, and four days respectively; the total number of tests was 337; the average number of additions accomplished in a test was 751. The total number of acts of addition which I examined was, therefore, about 253,087.

The number of written errors among these 253,087 acts of addition was (4,273 uncorrected, and 7,264 corrected) 11,537.

Many of the written digits were indecipherable, and many were rewritten, especially by persons who improved the figure forms they first wrote. The tendency of some of the investigated was to minimize their alleged mistakes; to interpret the indistinct sums as badly written but accurate results; and to explain corrections as improvements, not alterations. Only decipherable corrected errors were used.

When an act lapsed, an error was certainly made. As most of these lapses occurred at the end of the minutes, when the calling of the word 'stroke' distracted attention from the addition; and, as reveries were sometimes indicated by the lapse of only one act; I did not add the omitted sums to the number of the errors. The errors were, thus, conservatively estimated. The corrected errors exceeded 7,264. 11,537 is, therefore, an index, not to the accuracy with which the 253,087 acts of addition were performed, but only to the number of incontrovertible and decipherable written errors which I examined.

The operation in the test is not merely simple addition. Upon the measure of adding is imposed the novel procedure of

so-called *continuous* addition which I have already explained. The ordinary adjustment for addition must, therefore, be modified to ensure this alteration of the usual manner of adding. As the summands are not isolated in pairs but are printed so as to follow one another down the columns at equal distances, another adjustment is necessary—an adjustment which determines that only the printed summands will be added, and these only in succession and in pairs. To minimize time and effort spent in writing, to make the test an index to the intellectual, not to the muscular processes involved, the one denoting ten in any sum which exceeded 9 was omitted in writing. In example 1, the sums, 11, 13, and 15, were therefore written, 1, 3, and 5, respectively. This *abbreviated ciphering* demands still another mental adaptation.

Ex. I.	*Test* Summands.	Sums in Abbreviated Ciphering.	*Interpretation* Summands, Summands, Sums.
	3		
		7	3 + 4 = 7
	4		
		1	4 + 7 = 11
	7		
		3	7 + 6 = 13
	6		
		5	6 + 9 = 15
	9		

The task, therefore, is somewhat intricate. Its complexity, and the stimulating to speed which I practiced by encouraging competition among the investigated persons, were both useful to my purpose, for both tended to diminish accuracy and to reveal errors which under more simple and familiar conditions would not have been disclosed.

To assist the interpreting of the results obtained with abbreviated ciphering, eight persons were examined for five days each and completed 19,213 acts of addition, writing all numbers in full; thus:

Ex. II.	*Test*		*Interpretation*
	7		
		13	7 + 6 = 13
	6		
		15	6 + 9 = 15
	9		

2,614 reckoning acts were obtained from three persons, who were each examined for two days without the use of either abbreviated ciphering or of the continuous method, thus:

Ex. III.	*Test*		*Interpretation*
	7		
		13	$7 + 6 = 13$
	6		
	9		
		13	$9 + 4 = 13$
	4		

Figure Associations in General.—Study of the errors brings sharply before us certain properties of the associations existing between figures to which I may here for a moment refer.

One of the most primitive figure associations is the association of sequence. Digits in sequence are primitive engraved associations. Another fundamental association is that of factors. Factors partake also of the peculiarities which belong to the most powerful of the associated figure groups. Every act of addition is concerned with three chief digits, the two summands and their sum; the three together constitute an *associated figure group*. The number of mutual associations enjoyed by the digits of a group is an index to the associative strength of the group. Thus, the digits 3, 5, and 8 are very specially related. $8 + 5 = (1)3$; $8 - 3 = 5$; $5 \times 3 = (1)5$; $3 \times 5 = (1)5$; $8 - 5 = 3$; $3 + 5 = 8$; $5 + 3 = 8$. Eight possible combinations of two of these digits are thus associated with the third of them. 3, 5, and 8 have therefore an exceptional number of mutual associations; the group 3 5 8 has, therefore, considerable associative strength.

4 5 9 is an analogous group, and has the same associative peculiarities as 3 5 8. ($4 + 5 = 9$; $5 + 4 = 9$; $9 - 4 = 5$; $9 - 5 = 4$; $9 + 5 = (1)4$; $5 + 9 = (1)4$; $5 \times 9 = 45$; $9 \times 5 = 45$.)

A group of three digits, such as 3 5 8, which represents eight completed acts, we may distinguish as an *octad group*. As 4 5 9 represents seven acts, it may be termed a *heptad group*. 7 2 9 is a *quadrad group*. All other groups can thus be conveniently distinguished according to the number of their mutual associations, according to their associative strength.

Errors and Their Mechanism.—We shall first examine the errors which occurred with the digits 3, 5, and 8. Every error which will be examined occurred frequently, but for the sake of simplicity and clearness, I shall cite the minimum number of examples which will suffice to make plain the nature of whatever particular error is under discussion. Errors of a character similar to those with 3 5 8 happened with other associated figure groups; but by examining mainly errors arising from this one group, 3 5 8, we shall make the subject simpler to expound.

Suppression.—If the summands be distinguished as a, b, c, d, the method used in this reckoning test requires that after the act, $a + b = \overline{a + b}$, is completed, b be associated with c to yield $\overline{b + c}$; similarly, c must then be associated with d to yield $\overline{c + d}$; thus:

Ex. IV.

$$\begin{array}{ll} a & \\ & = \overline{a + b} \\ b & \\ & = \overline{b + c} \\ c & \\ & = \overline{c + d} \\ d & \end{array}$$

Ex. V.	Test		Interpretation
	(*a*)[1] 3		
		8	$3 + 5 = 8$
	(*b*) 5		
		12?	$5 + 7 = 12$
	(*c*) 8		
		15	$8 + 7 = 15$
	(*d*) 7		

Here the sum 8 $(a + b)$ was correctly suppressed. Although the strength of the tie which binds 5 and 8 is indisputable, yet 5 (b) was not added to the summand 8 (c)—as the task demanded—but to the summand 8's successor (d), the summand 7; the error 12 resulted. The sum 8 $(a + b)$ must, therefore, have been actively suppressed. Only thus can we explain the ignoring of the summand 8 (c). For the summand 8 thus to be ignored, its form must have been recognized, at

[1] The letters are not part of the printed text. They are inserted here only to facilitate the description.

least to the extent necessary to establish the identity of the printed summand 8 with the written sum 8. *Suppression may affect the digit suppressed so that the seeing of that digit results only in the associations necessary for the detection of its form,* and not in those essential to the customary linking of the digit seen with its figure association. Note that 3 and 5 yielded a motor response 8, although 8 occurred as the next summand (cf. example VIII.).

Ex. VI.	*Test*		*Interpretation*
	(*a*) 3		
		8	$3 + 5 = 8$
	(*b*) 5		
	(*c*) 7		
		11	$7 + 4 = 11$
	(*d*) 4		

Here $3 + 5 = 8$ was correctly accomplished; then the act $(b + c = \overline{b + c})$ $5 + 7 = 12$, not only failed, but an hiatus occurred in the reckoning; the whole act lapsed. The suppression of 8 ($a + b$) and 3 (a) suppressed the 5 (b) also. Any fresh perception of the 5 after the act $a + b = \overline{a + b}$ proved ineffective. At this moment, 7 was apparently completely shut out from the figure associations, for it was not even copied (see examples VII., XX.). *Hence, on the completion of an addition act, all associations of the digits implicated in that act may be suppressed completely.*

Ex. VII.	*Test*		*Interpretation*
	8		
		11	$8 + 3 = 11$
	3		
		7?	$7 = 7$
	5		
		12	$5 + 7 = 12$
	7		

Here 8 and 3 were correctly added, and 3 and 5 yielded no sum. This we know could not be due to lack of associative affinity between 3 and 5. The suppression of $8 + 3 = 11$ must have suppressed 5. Probably the essential cause was the suppression of (a) 8, the sum of 3 and 5 (cf. example V.).

Example VII. proves that *the suppression of a digit tends to suppress also the association of those digits habitually associated with it;* that suppression affects the concept of a digit and all its associations; and that, although introspection encourages the supposition that the dissociation of b from the act $a + b = \overline{a + b}$ is attained by the fresh perception of b associated with c, yet any such retroactive inhibitory influence upon $a + b = \overline{a + b}$, must be preceded, at least in certain cases, by an active dissociation of b from the other digits of the just completed act; or by the cessation of the suppression.

Ex. VIII.	*Test*		*Interpretation*
	(*a*) 7		
		12	$7 + 5 = 12$
	(*b*) 5		
		9?	$? = 9$
	(*c*) 3		
		11	$3 + 8 = 11$
	(*d*) 8		

Here 7 and 5 were correctly added. Then 9 was written where 8 should have been. *The motor expression of the sum of 5 and 3 was suppressed before it was written.* The suppression of 8, therefore, was not *the result* of a motor reaction. The premature suppression of the act $5 + 3 = 8$ before any motor reproduction was achieved was due to the perception together of the summands 5, 3, and 8 (b, c, and d). *The suppression emanated from the antemotoric figure associations;* it inhibited the reproductive associations of the digits of the completed act; it inhibited the reproduction of 8 as the sum of 5 and 3.

Whence came the 9 written instead of 8 $(\overline{b + c})$? Among the summands, 7 (a) and 8 (d) occur. 7 8 9 are in sequence. Summand 7 and sum (1)2 might, by addition, yield $(7 + 2)$ 9. There are two tendencies discharged by the writing of 9; an addition $(7 + 2)$ and a sequence (7 8 9) tendency. The suppression of 8 was incomplete, but its figure associations were limited to the reinforced elementary relation of sequence.

Ex. IX.	*Test*		*Interpretation*
	3		
		8	$3 + 5 = 8$
	5		
		9?	$? = 9$
	7		
		13	$7 + 6 = 13$
	6		

Here the act 5 + 7 had no motor expression. Instead, 9 was written. 9 could not have arisen from any addition of these digits except 3 and 6. But 5 6 7 8 9 are in sequence. Suppression permitted the figures to associate in the elementary relation of sequence. 9 conforms to addition, to sequence, and to factors (3 6 9). The writing of 9, therefore, discharged three tendencies.

Ex. X.	*Test*		*Interpretation*
	3		
		8	3 + 5 = 8
	5		
		10?	3 + 7 = 10
	7		
		16	7 + 9 = 16
	9		

Here again 5 and 7 were not added. 10 was written instead of 12. 3 + 7 = 10; but 10 is not merely a sum; it continued the sequence 7 8 9. The act 5 + 7 had no motor expression. There are two tendencies discharged by the writing of 10, an addition (3 + 7), and a sequence tendency (7 8 9 10).

When digits are improperly suppressed, the association in pairs and the set for addition partially fail. A superficial, an external, a primitive figure association, such as sequence, may occur, when the more elaborate or internal figure associations are thus suppressed. Note that when these external associations are reproduced, they generally discharge the affect of more than one associative tendency.

Ex. XI.	*Test*		*Interpretation*
	8		
		11	8 + 3 = 11
	3		
		5?	8 + 7 = (1)5
	1		8 − 3
		8	
	7		1 + 7 = 8

Here the act 8 + 3 = 11 was suppressed. Its suppression affected the summands 3 and 1. 3 was, therefore, not added to 1 to yield 4. 5 resulted instead. The error, 5, emphasizes the tendency of any two (8, 3) of an associated group (3 5 8) to elicit the third (examples IX. and X.); and the tendency of

the motor response under complicated conditions to purge the association sphere of all disturbing associations; the written response is thus the resultant with the greatest affect.

But 11 here induced the suppression of 1. The visual image of 1 must therefore be an integral part of the image of 11. In the visual form associations, 11 must be represented not by a special form, but by a duplication of 1. The suppression of sums, such as 13, 14, 15, 16, or 17, may be observed to suppress the succeeding summands, 3, 4, 5, 6, 7, respectively. Numbers greater than 9, numbers represented by more than one digit, have no individual visual form associations, but are compounded of the visual images of 0 to 9.

Ex. XII.	*Test*		*Interpretation*
	3		
		8	$3 + 5 = 8$
	5		
		16?	$8 + 8 = 16$
	8		
		9	$8 + 1 = 9$
	1		

Here $5 + 8 = 13$ should have occurred. Instead, 16 was written. The sum 8 should not have been added at all; yet it *was* added to the summand 8, for 16 was written instead of 13.

Ex. XIII.	*Test*		*Interpretation*
	(*a*) 3		
		8	$3 + 5 = 8$
	(*b*) 5		
		8?	$8 = 8$
	(*c*) 8		
		4	$8 + 6 = 14$
	(*d*) 6		

Here the suppression of 3, 5, and 8 (*a*, *b*, and $\overline{a + b}$) prevented the proper linking of 5 (*b*) and 8 (*c*) to yield 13 ($\overline{b + c}$); and caused the summand 8 (*c*) to be copied.

The 8, in this example, owes its reproduction to its duplication. Under similar circumstances, in example XII., the two 8's were added to yield 16; and, in example V., the second 8, the summand 8, was ignored. 5 and 3 under similar circumstances are subject to analogous effects.

The reproduction of 8 may be deemed an external associa-

tion. Mere duplication of a digit was never observed unaided to cause copying; invariably some objective figure arrangement which was demonstrably capable of eliciting a suppression fault was present at the same time. The evoking of 16 by two 8's is an addition association, and a factor association. Copying, sequence results, and the addition of identical digits are more primitive and more external figure associations than those required, as a rule, by the task.

When 8 was ignored, its associations must have been suppressed to a greater degree than when 8 was copied. When the two 8's were added, their associations must have been more numerous than when 8 was merely copied. Hence, suppression may vary in degree.

Ex. XIV.	*Test*		*Interpretation*
	(*a*) 3		
		8	$3 + 5 = 8$
	(*b*) 5		
		15?	$8 + 7 = 15$
	(*c*) 7		
		11	$7 + 4 = 11$
	(*d*) 4		

Here the written sum 8 was not suppressed, but associated with the summand 7 to yield 15. An addition association which contains 5 (*b*) was evoked. The addition of the sum $\overline{a + b}$ to the summand *c* may occur also when the result does not contain either *a*, *b*, or $\overline{a + b}$, (3, 5, or 8).

Numerous examples show that the most difficult digit to suppress of any three, of which two are related to the third as summands to sum, is the sum (examples VIII., IX., X., XII., XIV.). The summands *a* and *b*, persist less often perhaps because in the writing of the sum $\overline{a + b}$, the affects of *a* and *b* are in part discharged. The persistence of the sum $\overline{a + b}$ is due sometimes, as is seen with the sum 8 of 3 5 8, to the strength of the resistance to suppression engendered by the many associations of the sum and its summands. The persistence of the sum may be due to its concept being elicited by an intellectual image, reinforced by an objective motor image and strengthened by an objective visual image. The examples in which the sum persists to add itself to an identical

succeeding summand or to yield with a succeeding summand, a result which is in sequence, show that the efficacy of the suppression may be influenced by the degree to which the digit to be suppressed habitually associates with the summands which succeed it.

Failure of Suppression: Perseveration

Ex. XV.	*Test*		*Interpretation*
	3		
		8	$3 + 5 = 8$
	5		
		13?	$8 + 5 = 13$
	7		
		8	$7 + 1 = 8$
	1		

Here 13 was written instead of 12. 5 and 7 were not added. The 7 was ignored. 8 was added to 5 to yield 13, a correct addition, but uncalled for by the task. Then 7 and 1 were correctly added. The act of adding 8 and 5 replaced the adding of 5 and 7. The associations of the group 3 5 8 perseverated and achieved a superfluous motor expression.

Ex. XVI.	*Test*		*Interpretation*
	3		
		8	$3 + 5 = 8$
	5		
		11?	$8 + 3 = 11$
	7		
		16	$7 + 9 = 16$
	9		

Here 11 was written in place of 12. The 7 was again ignored and the act $8 + 3 = 11$ replaced $5 + 7 = 12$. $8 + 3 = 11$ is superfluous and is an error not in addition but in the order of adding.

In examples XV. and XVI., addition in pairs persisted. No hiatus (cf. example VI.) occurred in the reckoning. The combination of 5 with 7 was stillborn. These last two examples show that the digits of this group 3 5 8, in contravention of the existing set for addition of the printed summands alone, and of successive summands only, may perseverate, may evoke more than one motor manifestation. The combining of the digits 8 and 5, and 8 and 3 respectively, in the group 3 5 8

was more powerful to excite a motor response than was either the correct association of 5 and 7, or any combination of the digits of the group 3 5 8 with 7. *The 3 5 8 associations in these two examples perseverated.*

As we have seen, and as we shall repeatedly see throughout this paper, the addition of 5 to 7 under certain circumstances is attended by many and diverse errors. To combine 5 with 7 sufficiently to evoke a motor expression common to both is fraught with particular difficulty in these examples. In these examples, 5 is conjoined with the group 3 5 8. 5 not occurring with this group or with an analogous group, such as 4 5 9, combines, as a rule, freely and correctly with 7.

In examples XV. and XVI., suppression failed and the associated group 3 5 8 achieved a superfluous second motor expression. Digits such as 3 5 8, 4 5 9, 3 6 9, etc., which are habitually linked together, tend not only to be suppressed together, but by virtue of their intimate associations tend to resist suppression to an unwonted degree.

In example VII., the suppression of 8 and 3 suppressed also the 5. The more intimately three digits are associated, the more does the suppression of any one tend to suppress the other.

When the reckoner sees two summands, the written result depends not only upon the integrity of the fixed association which links these two summands, not even upon the ease with which these summands associate, but, at least, in part, also upon the force, the accuracy, and perhaps the duration of the suppression of the digits to be discarded from the preceding act of addition. The force and the accuracy of the suppression of the digits to be discarded depends upon the intimacy of the mutual relations between these digits and upon the affinity of the digits suppressed to the succeeding *already seen* summands. *The more numerous the associations of a digit, the more difficult is the suppression of that digit.*

Anticipations and Repetitions

Ex. XVII.	*Test*		*Interpretation*
	2		
		5	$2 + 3 = 5$
	3		
		2?	$5 - 3$
	8		
		2?	$3 + 9$ = (1)2
	3		
		2	2
	9		
		4	$9 + 5 =$ (1)4
	5		

3 and 8 were not added to yield 11. Instead, 2 was written and *thrice repeated;* once in place of $3 + 8 =$ (1)1, once in place of $8 + 3 =$ (1)1, and once in place of $3 + 9 =$ (1)2. The suppression of the act $2 + 3 = 5$ must have affected 3 and 8, for neither 3 and 8, nor 8 and 3 yielded a motor response. Apparently, 3 5 8 were associated and 8 was not written as a sum because it already appeared as a summand. Of 2, 3, and 5, 3 and 5 were more affected by the suppression than 2, for 2 was written thrice.

Ex. XVIII.	*Test*		*Interpretation*
	3		
		8	$3 + 5 = 8$
	5		
		9?	
	7		$7 + 2 = 9$
		9	
	2		

Here an error has arisen from the difficulty of linking 5 with 7 after the group 3 5 8. No response was yielded by 5 and 7; but instead, 7 was prematurely added to 2 to yield 9; and the sum 9 was repeated. Note 9 is also in sequence (7 8 9).

Ex. XIX.	*Test*		*Interpretation*	
	(*a*) 5			
		3	$5 + 8 =$ (1)3	
	(*b*) 8			
		2?	$5 - 3 =$	
	(*c*) 3			(1)2
		2	$3 + 9 =$	
	(*d*) 9			

Here the suppression of 5 (a), 8 (b), and 3 ($\overline{a+b}$), suppressed also 8 (b). 8 (b) and 3 (c) yielded no sum; 11 did not appear. The 3 (c) must also have been suppressed, because it was neither copied nor added to any of the preceding digits. The 2 written instead of 11 ($b + c$) may have arisen from $3 + 9$ ($c + d$) $= 12$, being prematurely added and repeated, or from $5 - 3 = 2$. Anticipation and repetition may both have occurred here. The erroneous repetition of a sum (such as 2 or 9) is not rare in reckoning. I think that when a state of psychomotor irritability exists and no fresh motor picture is stimulated, repetition of the just written figure occurs. The affect of the motor figure picture evoked seems to be incompletely discharged by the writing of a single motor reproduction. Usually an association reinforcing the repeated motor picture (7 8 9 in example XVII. and $5 - 3 = 2$ in example XVIII.) is present.

A similar phenomenon may be seen in the speech of the excited.

When an hiatus occurs, as in example VI., either the psychomotor excitability is less, or the affect of the motor figure picture is less. Perseverations, anticipations, faulty suppressions, and the other errors shown with the group 3 5 8 are equally demonstrable with the groups 4 5 9, 3 6 9, 2 4 8, etc.

The Influence of the Objective Stimuli upon the Attention.—We have already seen that of the digits in the span of apprehension, those specially related tend to be perceived, to be associated together despite the "set" for addition. In the succeeding examples, we shall trace this influence more closely.

Ex. XX.	*Test*		*Interpretation*
	3		
		8	$3 + 5 = 8$
	5		
		7?	$? = 7$
	7		
		9	$7 + 2 = 9$
	2		

3 and 5 were correctly added. The task then required the adding of 5 and 7. Instead of 5 and 7 yielding 12, 7 was written. 7 was written after the digits 3, 5, and 8. No combination of these digits alone or with 7 can yield 7.

First 3 and 5 were added to yield 8, and second, 7 and 2, to yield 9, their respective sums. Two acts of correct addition occurred, one immediately before and one immediately after the error—the writing of 7.

The writing of 7 in error must, therefore, have been due to causes which arose between these two correct acts and must have endured only for the single false act. *The cause of the disturbance must have preceded the error and must have ceased with the error.*

Almost invariably errors are thus isolated. The error as a rule completely discharges the *affect* of the association tendencies which are contrary to the task.

Among ninety-three errors which I examined, which occurred with 8 5 3, 8 3 5, 5 3 8, 3 5 8, and 3 8 5, as successive summands not succeeded by 7; and which were the only decipherable errors associated with the digits 8, 5, and 3 as summands, 7 did not appear. Hence, we may conclude that the writing of 7 in example XX. did not arise from any combination or association of the digits 3, 5, and 8 together.

In the twenty-three examples, all I collected, of errors associated with the successive summands 3, 5, and 7, and 5, 3, and 7, every error was traceable to these digits. No error arose in which an extraneous digit, a digit not derivable from one or more of those concerned, appeared.

As no combination of 3, 5, and 8 will yield 7; as 7 is not derivable from 3, 5, and 8; and as digits extraneous and unrelated to those seen are not written, the 7 written in example XX. must have been derived from the 7 seen; in spite of the existing set to associate digits in pairs and to reproduce their sum, the 7 seen *must have reproduced itself.*

3 5 8 is only one of several groups in connection with which this phenomenon may be observed.

Ex. XXI.	*Test*		*Interpretation*
	4		
		9	4 + 5 = 9
	5		
		7?	7 = 7
	7		
		11	7 + 4 = 11
	4		

Here with the group 4 5 9, 7 is copied as in example XX. This group has the same associative peculiarities as 3 5 8.

Ex. XXII. *Test*		*Interpretation*
3		
	9	$3 + 6 = 9$
6		
	7?	$7 = 7$
7		
	13	$7 + 6 = 13$
6		

Here with 3 6 9, 7 is again copied. The group 3 6 9 is also analogous in its associative properties to 3 5 8. Analogous responses to digits other than 7 are made with these and other groups.

The more closely a group of digits is associated, the more isolated must every member of that group be from all other digits. Hence, a digit alien to, and immediately following, an associated group (7 succeeding 3 5 8, example XX.) tends to be isolated and copied, and the motor manifestations of an associated group tend to continue (examples XV. and XVI.). The perseverative errors in examples XV. and XVI. are, therefore, due, at least partly, to the strength of the association which binds 5 to the group 3 5 8. The strength of association conferred by the task upon 5 and 7 in these instances was feebler than that which linked 5 to the group 3 5 8.

Under identical circumstances 7 was reproduced (example XX.), 7 was ignored (examples XV. and XVI.), and the result of the addition of 7 to its succeeding summand was anticipated (example XVIII.).

When 7 was ignored, its difference from 3, 5, and 8 must have been appreciated. Its visual image must have been established, at least to the extent necessary to detect this difference. *The identification of difference in the visual form of digits must, therefore, require weaker associative strength than is necessary to elicit the figure associations essential to motor reproduction.*

As 7 can be passed over during reckoning (examples XV. and XVI.), the objective visual stimulus must excite figure associations, at least, to a certain minimum degree, to procure

either the motor reproduction of a digit or any motor effect whatsoever. The stimulus to reproduction of a digit during reckoning must therefore depend upon the strength of the figure associations.

When 7 was reproduced, its recognition must have been more complete than the mere establishing of its difference from the digits 3, 5, and 8.

When 7 was added to its succeeding summand and achieved a premature result or participated in a sequence, its successful self-assertion must have been due to the strength of its associations with the already seen, simultaneously perceived digits.

From these examples, it is obvious that the figures seen are not limited to those which should be actively concerned in the addition at any given moment. Figures do not strike the retina singly. A certain small number are seen sufficiently to permit recognition of their form and relative position. Within this "span of apprehension," digits between which special associative properties exist tend to react on account of these properties, despite any existing set of contrary associative tendencies. In example VI., 3 and 5 were linked to the exclusion of 7; in example VII., 8, 3, and 5 were linked; in example VIII., 5, 3, and 8 were linked; in example IX., 7 was not linked with 5 but with 8 and 9; in example XX., 7 was isolated and copied; in example XVIII., 7 was linked with the succeeding 2 and achieved a premature result. Hence, we may conclude *that digits which habitually associate together, tend to be perceived together;* that the associative strength of a pair of digits is evident even in the linking of the visual images of these digits. Hence, what I may call scouting sometimes results:

Scouting:

Ex. XXIII.	*Test*		*Interpretation*
	3		
		8	$3 + 5 = 8$
	5		
		10?	$5 + 5 = 10$
	7		
		12	$5 + 7 = 12$
	5		
		14	$5 + 9 = 14$
	9		

Here no response was yielded by 5 and 7; but instead, 5 was added to 5 to yield 10.

Ex. XXIV.	*Test*		*Interpretation*
	2		
		8	$2 + 6 = 8$
	6		
		13?	$8 + 5 = 13$
	5		
		9	$5 + 4 = 9$
	4		

Here the sum 8 should have been suppressed and the summands 6 and 5 should have been added to yield 11. Instead, 8 persisted, associated with 5, yielded 13, and completed the group 3 5 8.

Ex. XXV.	*Test*		*Interpretation*
	2		
		5	$2 + 3 = 5$
	3		
		8?	
	6		$\left.\begin{array}{l} 5 + 3 \\ 2 + 6 \\ 3 + 5 \end{array}\right\} = 8$
		13?	
	5		
			$8 + 5 = 13$

Here the sum 5 should have been suppressed and the summand 3 should have been added to 6 to yield 9. Instead, 5 persisted, combined with 3, yielded 8, and completed the group 3 5 8. A precisely analogous error immediately followed; the sum 8 was added to the summand 5 to yield 13. The group 3 5 8 was here twice completed in contravention to the set for the task.

Examples XXIV. and XXV. show that *when any two of the three* digits, 3, 5, and 8, occur, the third is apt to be written, in spite of the established order of the test. Numerous examples prove the force of mutual attraction exerted by digits related as are these three.

We saw that 8, 5, and 3 have an exceptional number of mutual associations. Hence, the more numerous the associations linking two digits, the more inevitable is their association when both are seen. And the more inevitable their association, the more inevitable is a result not peculiar to either, but common to both.

The behavior of the groups 3 5 8, 4 5 9, 3 6 9, etc., proves that the more numerous the associative acts represented by three digits, *i. e.*, the greater the number of combinations of two of three digits expressed by the third, the greater is the tendency of any two of these digits to elicit the third, and the less their tendency to associate with an alien digit (examples XXIV. and XXV.).

Just as any two digits of a group, such as 3 5 8 or 4 5 9, tend to elicit the third of the group, so does the occurrence of digits in sequence or digits which are factors, tend to elicit a result continuing the sequence or factors. We shall here illustrate only the sequence errors.

Ex. XXVI.	*Test*		*Interpretation*
	4		
		11	$4 + 7 = 11$
	7		
		16?	$? = 16$
	8		
		17	$8 + 9 = 17$
	9		

The digits 7 8 9 were already in sequence. In this example the sequence is continued, not completed. Here the 16 might have arisen from $7 + 9$. The digit 6 in sequence with 7 8 9 is a component of 16. The erroneous sum represents an addition result wholly, and a sequence result partially.

Ex. XXVII.	*Test*		*Interpretation*
	8		
		13	$8 + 5 = 13$
	5		
		14?	$8 + 6 = 14$
	6		
		15?	$? = 15$
	8		

Here the 15 could have arisen from no other mechanism than the continuation of the sequence 13, 14, 15 in the sums. The sequence errors may have their source either in the summands or in the sums.

Ex. XXVIII.	*Test*		*Interpretation*
	7		
		9	$7 + 2 = 9$
	2		
		8?	$? = 8$
	5		
		9	$5 + 4 = 9$
	4		

The sum 8 cannot arise from the addition of any possible pair of digits here. It completes the sequence 7 8 9. Note that the factors 2, 4, 8 are now present.

Ex. XXIX. *Test*		*Interpretation*
2		
	9	$2 + 7 = 9$
7		
	6?	$9 + 7 = (1)6$
8		
	7	$9 + 8 = (1)7$
9		

Here the sum 9 was added to the summand 7 to yield (1)6. As the sums ran 6 7 and neither 8 7 nor 7 6, they arose from 9 + 7 and 9 + 8, respectively. Addition obviously persisted, as the sums are not 9 8 7 nor 9 8 9, but 9 6 7; and as the duplicated 9 is not written. Hence, addition, when it can produce a result conformable with the sequence, may prevail. In other words, a result peculiar to addition or to sequence may not occur when a result common to both is possible.

Ex. XXX. *Test*		*Interpretation*
9		
	7	$9 + 8 = (1)7$
8		
	8?	$7\ 8\ 9 = 8$
7		
	7?	$9 + 8 = (1)7$
6		

The sum 8 was copied as a result of the sequence. The subsequent sum 7 arose from 9 + 8. Had the sequence alone prevailed, (7 8 9), 9 would have been the result and not 7; had copying, 6 would probably have been copied.

Ex. XXXI. *Test*		*Interpretation*
8		
	5	$8 + 7 = 15$
7		
	9?	$8 + 1 = 9$
1		
	6	$6 = 6$
6		

Here, again, addition in erroneous order and copying were both associated with a continuation of the sequences 5 6 7 8 9.

The errors associated with sequence are additions in erroneous order, false sums, and repetition. The invariable result of these errors is that a sequence is continued (example XXIX.), or completed (example XXVIII.), or digits identical with those already written or printed in sequence are written (examples XXX. and XXXI.).

The sequence may be continued upwards or downwards or a member of the digits in sequence may be repeated out of order. I could not determine in which direction the sequence reaction most readily occurs. The tendency of summands in sequence is to stimulate concepts of digits in sequence, but which of the concepts finds motor expression may be determined by circumstances such as reinforcement by a sum from the submerged addition process.

In example XXIX., the sums run 9 6 7 and not 9 8 7; and in example XXX., 9 8 7 6 yield as sums, 7 8 7, not 7 8 9. Hence, the set for *addition in pairs* obviously persists alongside the tendency to sequence and if a result conformable with sequence and addition can arise, it does arise, in defiance of the order of the sequence.

We have seen that of the digits which assert themselves in the sequence 6 7 8 9, 6 and 7 may represent 16 and 17 and may thus conform both to addition and to sequence. But 16 and 17, the sums of 9 and 7, and of 9 and 8, respectively, do not conform to the sequence; only one component of their motor expression, the 6 of 16, and the 7 of 17, harmonize with the sequence. Only in the *motor* pictures of the two acts is there anything in common between them. In the motor pictures, no question of conflicting figure operations exists; a sequence result can reinforce an addition result, and the motor images of the component digits of numbers greater than ten are there represented separately. Numbers consisting of more than one digit, such as 16, are represented in the motor images by two separate associations, one for 1, the other for 6.

As addition conformable with the test may be evident only when a motor image common to the resulting concepts of the sequence and addition associations is present, we may suppose that the other unexpressed additions occur, that the

addition association is as inevitable as the sequence association. Conversely, when digits in sequence do not prevail over the set for addition, the sequence associations occur, but have a weaker affect than the addition associations.

The central fact which sequence mistakes teach *is that addition associations do not err;* only their motor expression errs; only their externalization is faulty. Their externalization is faulty partly because the motor responses, being few, are shared in common by more than one figure concept. Certain objective stimuli may awaken more than one association, more than one figure concept (examples XVII., XXV., etc.). The intensity with which any motor picture is evoked, obviously depends upon the strength of the affect which evokes it. The same motor picture may receive the affects of more than one figure association group. At the transference of the results of the internal figure associations into their equivalent motor images or in the externalization of that motor equivalent, either the motor image of the intrinsically dominant association, or the motor image which, being common to more than one association, is rendered dominant by the summation of the action of these several associations, prevails.

As addition conformable with the test, addition in erroneous order, and results conforming only to the sequence may all occur from the perception of the same group of digits, either the facilitation of the addition associations must be such that an addition result requires no appreciably greater length of time than is required for the result of the sequence; or the delay in the reproduction of figure concepts may suffice to permit the effect of the addition to act synchronously with the more rapid or more powerful association of sequence.

In reckoning disturbances, due to the objective appearance of digits in sequence, both the power of associating digits in pairs and the adding of digits may persist, alongside the tendency to sequence, and when a common motor result can be attained, it is attained. This suggests why 5 + 6 equalled (1)1, never 4 or 7; for their tendency to the proper written sum was augmented by the fact that 1 was also the difference between 5 and 6; this subsidiary association may have re-

inforced the dominant association. 4 + 5 usually gave 9, because these digits are particularly related in a group of greater associative power than the sequence tendency (cf. the isolation of 7 in association with this group, example XXI.). 8 and 9 practically always yielded (1)7, probably because of the united effect of the sequence association 7 8 9, and the addition association $9 + 8 = 17$.

Writing Errors.—A digit was sometimes falsely begun or falsely completed, or a merely purposeless stroke was written. Hence arose errors, for such false sums, although usually detected, sometimes escaped. The most frequent error of this class was 1 for 6 and 4; occasionally 0 for 9; sometimes 2 for 3, and vice versa. It is noteworthy how often these errors occurred in association with sequence and factors, conditions which I have shown cause a weakening in the set or a suppression error. It was when the rhythm of the associations was thus interrupted that the writing errors usually occurred. Commonly, the error was patently wrong, it did not fit any of the kindred sets,—addition,—sequence, factors, or copying. Hence, usually, it did not satisfy the visual critique and was consequently corrected. Occasionally two corrections of a sum, rarely even three were made and remained decipherable. But often the initial error was illegible under the correction, and sometimes even the correction could not be identified. Some of the seeming writing errors were merely sums badly written and subsequently improved. The writing error was therefore in part due to weakening of the set by the objective figure arrangement, in part to lack of attention, and in part to the psycho-motor excitability. The actual difficulty of writing varies with the different digits; thus, 1 is easier to write than 2; this variation also may be a factor in producing writing errors, under excitement.

We see from copying errors that every digit tends to excite its own reproduction. We see from sequence errors, that every digit must also tend to excite the figures contiguous to it; and that two digits in sequence excite a third in sequence. We can see from factor errors that two digits which are factors excite a third, which *may be* a quotient, a product, or a common multiple of the two, but *is* always a factor.

Irrespective of the order in which digits may be seen and in spite of a subjective facilitation for addition, visual images of digits which habitually associate have, by virtue of the strength of that association, an inherent power to direct the mental processes of reckoning. We have also seen that an active power of suppression of figure associations exists, and that the stronger the figure associations, the more do they resist suppression, and the more do they tend to be suppressed together. When the objective arrangement of digits (sequence, factors, etc.) weakens the set for addition, or the insistent associative tendencies of digits lead digits to resist suppression, the written response, the error, is determined not by accident, but by definite forces. These forces result in errors of the nature of omissions, perseverations, anticipations, etc. Certain errors seem to be peculiar to certain states. Thus, under the influence of alcohol, not only the number of errors increases, but errors of a certain nature seem to predominate. I hope further examination will show that the errors, by their nature and by their relative frequency, may afford us, what we now lack, a measure of the associative power which will be of diagnostic value.

Summary

1. The fundamental response to a perceived isolated digit is the reproduction of that digit.

2. This response, during reckoning, emanates from the figure association sphere, and is dependent upon the functional integrity of the figure associations.

3. Two different digits perceived together tend to produce not an effect peculiar to either, but a resultant common to both.

4. Not only recognition of their forms, but also association of the two digits is essential to the eliciting of a common resultant.

5. The greater the number of associations between two digits, the more inevitable is their association together.

6. The more numerous the associations between three digits, the more inevitable is the externalization of the third when the other two are seen.

7. Objective sequence may induce a response in sequence; objective factorization may induce a factor; objective duplication may induce copying; objectively suggested figure groups may be completed; the grouping of the digits when they are perceived may guide their further association, may dominate attention.

8. Suppression of digits is an active mental process and inhibits the association and the reproduction of the suppressed digits.

9. The suppression of a digit suppresses also the digits habitually associated with it.

10. The primitive attributes of a visual stimulus are not suppressed. Visual form is detected in spite of suppression. Among the figure associations, the most primitive are the most resistant to suppression; copying, sequence, and the adding of identical digits (factors) may survive when all other figure associations are absent.

11. The greater the number of associations of a digit, the more difficult is the suppression of that digit.

12. Digits which by virtue of their intimate associations resist suppression tend to continue their motor manifestations.

13. The motor manifestations of imperfectly suppressed digits depend partly upon the associative opportunities which exist at the moment. It is the result and not the operation which, in such cases, determines the motor response. That digit which most completely discharges the affect of the figure associations is written.

14. The nature and the frequency of the errors may serve as a measure of associative capacity.

Ex. I.

Test		*Interpretation*
3		
	7	$3 + 4 = 7$
4		
	1	$4 + 7 = 11$
7		
	3	$7 + 6 = 13$
6		
	5	$6 + 9 = 15$
9		

Ex. II.

Test		*Interpretation*
7		
	13	$7 + 6 = 13$
6		
	15	$6 + 9 = 15$
9		

Ex. III.

Test		Interpretation
7		
	13	$7 + 6 = 13$
6		
9		
	13	$9 + 4 = 13$
4		

Ex. IV.

Test	Interpretation
a	
	$= \overline{a + b}$
b	
	$= \overline{b + c}$
c	
	$= \overline{c + d}$
d	

Ex. V.

Test		Interpretation
3		
	8	$3 + 5 = 8$
5		
	12?	$5 + 7 = 12$
8		
	15	$8 + 7 = 15$
7		

Ex. VI.

Test		Interpretation
3		
	8	$3 + 5 = 8$
5		
7		
	11	$7 + 4 = 11$
4		

Ex. VII.

Test		Interpretation
8		
	11	$8 + 3 = 11$
3		
	7?	$7 = 7$
5		
	12	$5 + 7 = 12$
7		

Ex. VIII.

Test		Interpretation
7		
	12	$7 + 5 = 12$
5		
	9?	$? = 9$
3		
	11	$3 + 8 = 11$
8		

Ex. IX.

Test		Interpretation
3		
	8	$3 + 5 = 8$
5		
	9?	$? = 9$
7		
	13	$7 + 6 = 13$
6		

Ex. X.

Test		Interpretation
3		
	8	$3 + 5 = 8$
5		
	10?	$3 + 7 = 10$
7		
	16	$7 + 9 = 16$
9		

Ex. XI.

Test		Interpretation
8		
	11	$8 + 3 = 11$
3		
	5?	$8 + 7$ / $8 - 3$ $= (1)\ 5$
1		
	8	
7		
		$1 + 7 = 8$

Ex. XII.

Test		Interpretation
3		
	8	$3 + 5 = 8$
5		
	16?	$8 + 8 = 16$
8		
	9	$8 + 1 = 9$
1		

Ex. XIII.

Test		Interpretation
3		
	8	$3 + 5 = 8$
5		
	8?	$8 = 8$
8		
	4	$8 + 6 = 14$
6		

Ex. XIV.

Test		Interpretation
3		
	8	$3 + 5 = 8$
5		
	15?	$8 + 7 = 15$
7		
	11	$7 + 4 = 11$
4		

Ex. XV.

Test		*Interpretation*
3		
	8	$3+5=8$
5		
	13?	$8+5=13$
7		
	8	$7+1=8$
1		

Ex. XVI.

Test		*Interpretation*
3		
	8	$3+5=8$
5		
	11?	$8+3=11$
7		
	16	$7+9=16$
9		

Ex. XVII.

Test		*Interpretation*
2		
	5	$2+3=5$
3		
	2?	$\left.\begin{matrix}5-3\\3+9\\2\end{matrix}\right\}=2$
8		
	2?	
3		
	2	
9		
	4	$9+5=(1)4$
5		

Ex. XVIII.

Test		*Interpretation*
3		
	8	$3+5=8$
5		
	9?	
7		
	9	$7+2=9$
2		

Ex. XIX.

Test		*Interpretation*
5		
	3	$5+8=(1)3$
8		
	2?	$\left.\begin{matrix}5-3=\\3+9=\end{matrix}\right.(1)2$
3		
	2	
9		

Ex. XX.

Test		*Interpretation*
3		
	8	$3+5=8$
5		
	7?	$?=7$
7		
	9	$7+2=9$
2		

Ex. XXI.

Test		*Interpretation*
4		
	9	$4+5=9$
5		
	7?	$7=7$
7		
	11	$7+4=11$
4		

Ex. XXII.

Test		*Interpretation*
3		
	9	$3+6=9$
6		
	7?	$7=7$
7		
	13	$7+6=13$
6		

Ex. XXIII.

Test		*Interpretation*
3		
	8	$3+5=8$
5		
	10?	$5+5=10$
7		
	12	$5+7=12$
5		
	14	$5+9=14$
9		

Ex. XXIV.

Test		*Interpretation*
2		
	8	$2+6=8$
6		
	13?	$8+5=13$
5		
	9	$5+4=9$
4		

Ex. XXV.

Test		*Interpretation*
2		
	5	$2+3=5$
3		
	8?	$\begin{matrix}5+3\\2+6=8\\3+5\end{matrix}$
6		
	13?	
5		$8+5=13$

Ex. XXVI.

Test		*Interpretation*
4		
	11	$4+7=11$
7		
	16?	$?=16$
8		
	17	$8+9=17$
9		

Ex. XXVII.

Test		*Interpretation*
8		
	13	8 + 5 = 13
5		
	14?	8 + 6 = 14
6		
	15?	? = 15
8		

Ex. XXVIII.

Test		*Interpretation*
7		
	9	7 + 2 = 9
2		
	8?	? = 8
5		
	9	5 + 4 = 9
4		

Ex. XXIX.

Test		*Interpretation*
2		
	9	2 + 7 = 9
7		
	6?	9 + 7 = (1) 6
8		
	7	9 + 8 = (1) 7
9		

Ex. XXX.

Test		*Interpretation*
9		
	7	9 + 8 = (1) 7
8		
	8?	7 8 9 = 8
7		
	7?	9 + 8 = (1) 7
6		

Ex. XXXI.

Test		*Interpretation*
8		
	5	8 + 7 = 15
7		
	9?	8 + 1 = 9
1		
	6	6 = 6
6		

CHART OF THE ERRORS DISCUSSED IN THE TEXT.

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THE AFTER-EFFECT OF VISUAL MOTION

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Introduction

Problem and Point of View.—The past history of the present problem is long and complicated. I shall refer those who desire its presentation to the monographs of A. v. Szily[1] and A. Wohlgemuth.[2] There is one tendency which is present in practically all studies of visual after-movement. This is the persistent effort to reduce the causal factors to one only. I shall seek to remedy this defect of over-simplification by showing: (1) that there are at least three factors of great efficiency in the production of the illusion; and (2) that there are some cases where the after-effect takes place without all of the factors being active.

One of the three productive causes to which my experiments point is an eye-muscle strain due to the inhibited tendency of the eyes to follow moving lines. August Classen[3] had in mind a similar factor, but his discussion was vitiated (?) by the assumption of feelings of innervation through which the subject became aware of the tendencies to eye-movement. Purkinje,[4] Helmholtz,[5] J. J. Hoppe,[6] Stricker,[7] *et al.*,

[1] Szily, A. v., 'Bewegungsnachbild und Bewegungskontrast,' *Ztsch. f. Psych. u. Physiol. d. Sinn.*, 1905, Bd. 38.

[2] Wohlgemuth, A., 'On the After-effect of Seen-Movement,' *Brit. J. Psych.*, Mon. Supp., 1911, Vol. 1, No. 1.

[3] Classen, A., 'Ueber das Schlussverfahren des Sehaktes,' Rostock, 1863.

[4] Purkinje, J., 'Beobachtungen und Versuche zur Physiol. der Sinne,' Bd. 2, S. 60, 1825.

[5] Helmholtz, H. v., 'Handbuch der Physiol. Optik,' 2d ed., 1896, S. 764.

[6] Hoppe, J. J., 'Die Scheinbewegungen,' Wurzburg, 1879.

[7] Stricker, 'Ueber die Bewegungsvorstellung,' Wien, 1882.

have claimed that eye-movements of essentially a nystagmic character are set up and that these, persisting after the moving object had stopped, result in the illusion. The causal element which Classen and I defend is essentially different from this as will be seen later in the discussion. It is the same factor that Carr[1] and Adams[2] placed emphasis upon in their studies of the auto-kinetic illusion. The two other important elements in the production of visual after-movement are suggestion and the fading of after-images. The former of these, as here advocated, is more or less closely allied with the errors of judgment postulated by Zöllner[3] and Budde[4] and with the association factors claimed by Wundt.[5]

Subjects.—The data here presented were obtained from eight subjects. One was a professor of psychology. Two were instructors in English. Although untrained in psychology, they were excellent observers. Three were graduate fellows in psychology and one was an undergraduate who had had the conventional training course in experimental work. The remaining subject was the author of this paper.

Apparatus and Method.—The real[6] movement was produced in one instance by rotating a Scripture drum upon which were black strips $\frac{1}{2}$ in. wide and white strips $\frac{3}{4}$ in. wide. The drum rotated about a horizontal axis and was driven by an electric motor. The rate of movement was controlled by a rheostat and a speed reducer. Unless otherwise stated the drum was maintained at a rate of one rotation in 3.3 secs. This was a medium rate and gave a clear perception of wavy real motion and an after-movement nearly equal in length to the period of stimulation. A screen of

[1] Carr, Harvey, 'The Auto-kinetic Sensation,' Psych. Rev., Vol. 17, 1910.

[2] Adams, H. F., 'Auto-kinetic Sensations,' Psych. Rev., Mon. Supp., Vol. 14, No. 2, 1912.

[3] Zöllner, F., 'Ueber eine neue Art von Pseudoscopie und ihre Beziehung zu der von Plateau und Oppel beschreibenen Bewegungsphaenomenen,' *Poggendorf's Annalen*, Bd. 110, S. 500–23, 1860.

[4] Budde, E., 'Ueber metakinetische Scheinbewegungen und ueber die Wahrnehmung von Bewegungen,' *Arch. f. Anat. u. Physiol.*, Dubois Reymond, S. 127, 1884.

[5] Wundt, Wm., 'Physiol. Psychologie,' 6th ed., Bd. 2, pp. 614–24.

[6] The *real* movement, which is the actual movement of the external object, is to be contrasted with the *after-movement*, which is the apparent movement of the external objects due to the previous stimulation of the eye by the real movement.

white cardboard with an aperture $4 \times 7\frac{1}{2}$ in. was placed between the rotating drum and the observer. The screen was surrounded by a black cloth which served further to conceal the apparatus from view. Unless otherwise stated the subject was seated about five feet from the screen in such a position that the moving black and white lines were the only parts of the apparatus visible behind the screen. No head-rest was used. In those experiments where it was desired merely to describe the after-movement, as opposed to those where the purpose was to control this movement, the subject was instructed: to hold his head erect; to relax his muscles; and not to strain his eyes any more than was absolutely necessary in order to hold his fixation constant. The usual fixation points were dots placed in the middles of the upper and lower boundaries of the screen aperture. Where another fixation or none at all was used, due mention is made of the fact. In other tests a black spiral upon a white disc was used. The fixation point was the center of the disc. The after-effects were observed either upon the disc itself or upon a printed sheet.

Introspective Description of the After-Movement

Introspective characterizations of the after-effect of visual motion, when this motion is permitted to take its normal course without attempted interference by the observer, present data that are of considerable importance. I have brought together the following facts because of this and because of their bearing upon previous work in this field.

1. There are marked individual differences in the qualitative character of the after-effect. Many psychologically naïve subjects have been tested under conditions where the drum was rotated by hand rather than by the motor. The subjects interpreted the after-effect as a genuine rotation of the drum opposite in direction to the preceding movement. Some of the trained subjects also interpreted the after-movement as qualitatively similar to real rotation. Such an interpretation is at times replaced with all of the subjects by the feeling that the after-movement is a shadow movement,

unreal and ghostly yet nevertheless insistent and compelling. Some subjects took this view all of the time. One of the best trained observers persisted for several long periods in seeing the after-effect as a movement of something between himself and the drum which was known to be stationary. Depending upon the individual, then, and upon certain periods with the same individual, the after-movement may be interpreted as real, as a shadow or ghostlike movement on the drum itself, or as a movement of something between the observer and the drum,—this movement being contrary in direction to the real movement.

2. Various illusions of depth appear. None of the five subjects used in this test has failed to get this at some time during the experimentation. These illusions were much more prominent in work with the spiral. When rotated the spiral appeared to take the form of a cone with the point directed either away from or toward the subject. In the after-movement this third dimensional aspect was reversed. Where the after-movement was observed upon a printed sheet, it was this depth illusion and not the inward or outward flow which caught the subject's attention inevitably. In the case of the rotation of the parallel lines, the only illusion of depth which appeared was an occasional rapid receding of the drum area during the after-movement. This was not a rotation. The drum simply appeared to move farther away.

3. The after-movement does not always occur uniformly over the area which has previously been in motion. This is true whether the stationary area itself is observed or whether the after-effect is projected upon another surface. Later in this paper when treating of control, we shall have occasion to revert to this point. At present it must suffice to say that for some subjects the after-movement appears to break up into patches of movement about midway between its beginning and its ending. These patches of rest do not occur more frequently on the peripheral field than in central vision. Furthermore they do not always occur at the same relative position of the projection area. With one very excellent

subject, *e. g.*, the end of the after-movement has been most often described as a little patch of movement which varies in location from trial to trial. The direction of attention may be of importance here.

Another phenomenon deserves mention in this connection. All of the subjects found it difficult to say just when the after-effect stopped. There was a strong tendency for the movement to begin again when the subject offered the judgment 'stopped.'[1] Again—for some subjects more than for others—the after-movement had a tendency to be intermittent. It would appear, stop and reappear. The interval of rest was short. This paragraph should be read in the light of the control tests reported later in this paper.

4. I have sought for introspective characterizations of the nature of the stopping of the after-effect. Nothing has been secured that could be generalized. Some of the subjects regarded the stopping as a slowing up in the velocity of the movement. Others thought of it as a diminution in the intensity. The difficulty may well be entirely verbal. Either interpretation or both may be correct.

Theories of Explanation

In the consideration of a very old and much discussed problem, it is hardly necessary first to parade the experimental data and then to show their theoretical implications by repeating them in summary. In the present paper, I have chosen to group all of the observed facts accordingly as they support this or that explanatory rubric. Wohlgemuth suggests a division of the historical theories into physical, psychical and physiological. The classification has little intrinsic value, however, because the physical factors—eye-movements—reduce to physiological ones as do also the psychical ones where they are not claimed as overtly and explicitly conscious.

I think there can be little doubt any more that eye-

[1] A somewhat similar phenomenon has been described for the negative after-image of color by Thompson and Gordon (Thompson, Helen B., and Gordon, Kate, 'After-images on the Peripheral Retina,' 1907, Psych. Rev., Vol. 14, p. 132).

movements are not a factor in the after-effects of visual motion. This theory—supported notably by Helmholtz—would have it that the motion seen sets up a nystagmus which, persisting after the motion ceases, is interpreted as a movement of the objects in the opposite direction. The essential objections to this theory, I conceive, to be the following: (1) Wherever the real motion is seen going in two or more opposite or divergent directions, no nystagmus which might hypothetically be aroused can account for the after-movement which opposes in direction the real movement. If a nystagmus can cause an apparent movement, this would be in one direction only. Such a factor cannot account for the after-effects from the rotation of a spiral, nor can it account for the after-movement which occurs when the lines on the rotating drum are viewed both on the drum and on a mirror held below the drum. (2) In the experiments where the fixation is maintained, there is no evidence of a nystagmus, and yet the after-movement certainly appears. If the subject fixates a dot at the middle of the top to the aperture behind which the drum is revolving, any marked shift from this fixation is indicated by the appearance of a negative after-image of the relatively dark moving area upon the opposite side of the aperture. My subjects were warned to watch for this, and the results indicate that with minor exceptions the fixations were maintained. Tests were also made in which the subject's eyeball was observed through a high power reading glass during and after the real movement. No nystagmus was in evidence. Barany's[1] observations support this. In speaking (S. 205) of the nystagmus produced by *fixating moving* objects, he says: "Einen Nachnystagmus der beim Wechsel von der Fixation der äusseren Gegenstände zur Fixation eines im Wagen, also in scheinbarer Ruhe befindlichen Gegenstandes auftreten könnte, habe ich nie beobachtet." (3) Our discussion of this topic may go farther. Could a nystagmus, *if present*, produce an apparent motion of objects in one definite direction? No satisfactory explanation of such an occurrence has ever come to my

[1] Vide *infra*, p. 252.

notice. The problem has far-reaching complications, an adequate consideration of which must be deferred to another time. It is necessary, however, to indicate some of the difficulties involved because of the past history of this factor with respect to visual after-movement. Nystagmus, interpreted as a mere oscillatory eye-movement, can hardly explain the phenomenon. Either the afferent impulses set up in the eye-muscles or the retinal displacements of stationary objects due to the persistent nystagmus should produce an oscillatory (after) movement of external objects rather than a movement in one direction. It is worthy of note, however, that cases are on record where such an alternation of opposed movements of external objects has resulted after rotation of the subject upon a revolving chair. (Barany, S. 221.) On the other hand, a theory which uses nystagmus in the usual sense of a slow follow movement with quick recovery has its difficulties also: (*a*) Let us consider the problem first from the side of the retinal displacements involved. Holt[1] has presented experiments indicating a central anesthesia during rapid eye-movements. Accepting these data, one is led to conclude as Holt does in his later study of visual dizziness[2] that during the rapid phase of the nystagmus there is no vision either before or after the objective movement has stopped. Any retinal factors producing the after-movement must then be induced during the slow phase of the nystagmus. Now while the eyes are following a slowly moving series of objects, there is no essential retinal displacement and therefore no fading after-image streaks will be left in the eye. If the nystagmus set up by the movement of objects were the same as that set up when the body is rotated and external objects are stationary, then when the movement of the objects ceased, the rapid and slow phases of the eye-movement would be reversed in direction. The retinal displacements of external objects during the slow phase of the persistent nystagmus would be opposed to the direction really taken by the seen after-movement, *i. e.*, they

[1] Holt, E. B., 'Eye-Movement and Central Anesthesia,' Psych. Rev., Mon. Supp., Vol. 4, 1903.

[2] Holt, E. B., 'Vision During Dizziness,' Harvard Psych. Studies, Vol. 2, 1906.

would be in the same direction as the previous objective movement. If we do not assume that the nystagmus is reversed, then after the objective movement has ceased, the retinal displacements of stationary objects due to the slow phase will be in a direction suitable for the production of the after-movement actually seen. It is better to assume that the nystagmus is not reversed in as much as a reversed nystagmus seems to be correlated with a reversed vestibular stimulation. However the following reasons may be given why retinal displacements due to a non-reversed nystagmus could not condition the after-effect: (1) Where the nystagmic sweep of the eyes is wide, one should be able to duplicate the retinal displacements voluntarily. Such voluntarily initiated eye-movements do not result in the customary after-movement. This is taking for granted that a retinal displacement produced by a voluntary act has the same meaning as one produced by a reflex act. (2) If large movements cannot produce the required phenomenon, it seems unwarranted to assume that nystagmic movements of microscopic extent would do so by the retinal displacements involved. When Barany[1] claims that such minute movements may produce dizziness, he has simply stated the temporal coincidence of the two phenomena and has neither proved nor explained their causal connection. If we assume—as must be done—that the follow movements of the eyes do not keep up with the movement of the parallel lines when this proceeds at the rate used in the experiments of this paper, retinal displacements will result in fading after-images which are sufficient to produce an after-movement entirely independently of the nystagmus. (*b*) Let us consider the kinesthetic impulses set up by the nystagmus. Why should an oscillation of the impulse from the upper eye-muscles with the impulse from the lower eye-muscles produce an after-movement in one direction rather than two movements alternating in direction? Some one replies at once; "But, ah, the impulses arising from the two sets of muscles may vary in intensity and this may

[1] Barany, R., 'Untersuchungen über den vom Vestibularapparat des Ohres reflektorisch ausgelösten rythmischen Nystagmus und seine Begleiterscheinungen,' *Monatsschr. f. Ohrenheilk.*, Bd. 40, 1906, S. 214.

produce the after-movement." This is a valuable suggestion. However we must claim upon this basis that the less intense kinesthetic impulse (eye-muscle strain) is ignored and that the judgment is based upon the dominating strain. The work of Carr and Adams above referred to and my own experiments reported below show that the (after) movement occurs in the direction of the greatest strain. We are led to conclude, therefore, that the predominant strain is opposed to the direction of the real objective movement, *i. e.*, is opposed to the slow phase of the nystagmus. Now we are again in a position to maintain that the nystagmus *qua* nystagmus is irrelevant to the after-movement. It is the predominating eye-muscle strain that is important.

I referred above to the complicated nature of the present problem if it were followed out in all of its bearings. I cannot but mention one other point in this connection. Holt[1] in his study of ocular nystagmus was able to inhibit bodily dizziness by inhibiting the rapid phase of the nystagmus. This was accomplished by turning the eyes to an extreme position in the direction of the slow movement. Bárány in the work above mentioned (S. 224) was able to control both visual and bodily dizziness by the same method. (See my own control experiments below, p. 265.) In the light of data upon the present problem of visual after-movement, I think the explanation of this control lies in the fact that the strain in the new direction supported by association factors overcame the strain opposed in direction to the real movement. The result was an absence of movement. This point of view, I think, is preferable to Holt's more speculative theory of innervation processes. It also permits those who, like Professor Holt,[2] would find it hard to dispense with eye-movement sensations to keep that explanatory rubric.

There is another type of eye-movement which is not nystagmic, but which is always present in attempted fixations, viz., the involuntary eye-movements of small extent and irregular direction. Theoretically it may be held that

[1] Holt, E. B., 'On Ocular Nystagmus and the Localization of Sensory Data during Dizziness,' Psych. Rev., Vol. 16, pp. 390–1, 1909.

[2] *Ibid.*, p. 391.

these irregular eye-movements are interpreted as movements of the objects. A very similar doctrine has been advanced to account for the auto-kinetic sensation. But why should these irregular movements result in a fairly steady movement in one direction which is contrary to that of the stimulating movement? Such an hypothesis seems out of the question.[1]

The explanatory factors of large significance are retinal changes, association factors and eye-muscle strains.

Retinal Factors

If after watching a real movement for 20 secs., the subject turns his eyes toward a figured surface, he will see an after-movement which is limited in extent to an area corresponding to the retinal area stimulated by the original movement and which opposes in direction the original movement. This limitation of area is the great fact in support of an explanation of visual after-movement in terms of retinal factors as opposed to one in terms of the ocular muscles. In order to ascertain just how absolute this areal restriction of the after-effect was, I performed the following experiment:

1. One half of the aperture in front of the drum was covered with black paper. The subject was then instructed to fixate the upper dot[2] and to transfer the point of fixation to a dot on a printed sheet when the proper signal was given.

[1] For a discussion of this theory with reference to the autokinetic illusion see pp. 67–68, Carr, *op. cit.* I have not discussed the theory of special central movement processes in this paper, because the factors presented have seemed adequate to account for the phenomena and have also seemed less speculative. I find myself in agreement with much of the criticism which Henry J. Watt directs against Wohlgemuth's theory (Watt, Henry J., 'The Psychology of Visual Motion,' *Brit. Jr. Psych.*, 1913, Vol. 6, pt. 1).

[2] 'Upper dot' means the point of fixation in the middle of the upper side of the aperture. 'Lower dot' has a corresponding significance. I have made extensive tests upon 5 subjects in order to determine whether or not the direction up or down of the lines, or the fixation of the upper or lower dot had a peculiar effect upon the after-movement. A number of relations are here involved. If the upper dot is fixated and the drum moves downward, the lines not only go down, but they go away from the fixation point. If the lower dot is fixated, the lines still go downward, but they approach the fixation point. Similar relations hold for an upward rotation of the drum. I do not find that these changes have any effect upon the nature of the after-movement or upon the facility of controlling it.

These tests were carried out upon three subjects with uniform results from subject to subject. In as much as the screen surrounding the drum was white, it contrasted with the darkened area of the moving black lines so that under normal conditions the subject could project a negative after-image of the aperture upon the printed page. This negative after-image contained the after-movement within its boundaries. It had occurred to me that this restriction was due to suggestion from the size and form of the after-image. The nature of the surrounding objects had, in other words, inhibited the spread of the perception of motion. Placing the black paper over one half the aperture preserved the size and shape of the negative after-image and still prevented the stimulation of the retina by the movement over one half the area. Under such conditions the after-movement was still confined primarily to the area not covered by the black paper, but secondary phenomena appeared. The lines of print in the stationary area seemed to bend and be dragged into a participation with the moving part. Usually this was only evident at the boundary of the two areas, but at times the after-movement swept all of the area of the negative after-image along, even that in the part corresponding to an unstimulated retinal area. The explanation of this undoubtedly lies in the suggestion that if part of an unbroken line moves upwards the rest must do like wise. The participation of a factor of strain in the ocular muscles is also not to be over-looked.

I have not suggested what the nature of the retinal factor may be. The conventional theory at the present time is that of fading after-images. In the past, modified blood flow, displacement of retinal factors and other more or less mysterious factors have been proposed. Szily[1] and Schilder[2] describe a streaming phenomenon which occurs at right angles to the lines of the moving area. This has been pointed out before by Pierce[3] and is mentioned by Ferree[4] who,

[1] *Op. cit.*, S. 135–6.

[2] Schilder, Paul, 'Über auto-kinetische Empfindungen,' *Arch. f. d. ges. Psych.*, 1912, Bd. 25, S. 71–3.

[3] Pierce, A. H., 'Studies in Space Perception,' N. Y., 1901, pp. 331–8.

[4] Ferree, C. E., 'The Streaming Phenomenon,' *Amer. Jr. Psych.*, 1908, Vol. 19, p. 503.

however, holds that the phenomenon is not to be identified with that described by himself in his study of fluctuations of attention. The difficulty with an explanation in terms of the 'streaming phenomenon' lies in the limitation of the after-movement to a specific area. The objection to the other historical factors above mentioned lies in their mysteriousness. (This would be no real objection if facts could be adduced which demanded an explanation in retinal terms and which nevertheless were inconsistent with a theory of fading after-images.) The after-image theory does not encounter this difficulty. It is based upon the fact that after-images do fade in the direction of the moving stimulus. It assumes that this fading, although not directly perceived, leads to the interpretation that the stationary objects seen through the after-image flux are moving in the opposite direction. Wohlgemuth,[1] by way of maintaining his theory of a special central movement process, criticized the after-image theory as follows: By the fading of an after-image is to be understood a difference in the state of fatigue at two different parts of the retina. It is the recovery from fatigue which is designated by the term fading. Such a condition may well be postulated when a single stimulus passes across the retinal area. However, when a continuous succession of lines moves across the retina for the length of time used in securing after-movements, one part of the retinal area will be as fatigued as another. There can then be no fading and hence, on this theory, no after-movement. Wohlgemuth overlooks the fact that if such a uniform state of fatigue were secured, the subject would get a mixture of the black and white lines or at least a flicker and would not see a rotation or moving of the drum. It is customary to explain color mixture on this very basis of 'fatigue.' His own experiments prove that unless real movement is seen (*i. e.*, unless the rate of rotation is slower than that required for flicker) no after-effect can be obtained.[2] At this time the present writer is willing to view the retinal factors as most probably of an after-image nature. The terminology of the after-image theory is used in this

[1] *Op. cit.*, pp. 92–95.

[2] *Op. cit.*, p. 28.

paper, but the author is not convinced that some other so-called mysterious factors may not be very influential. In fact, he has experimental data which at present would appear to point to such factors. Further work is necessary before presenting this material. A brief statement, however, will be found on p. 275 of this paper.

In addition to the above experiment, the following have been performed and have yielded results supporting the theory of retinal causation.

1. If the subject observes the real movement of the drum with one eye and then watches the stationary drum or a printed sheet with the other *unstimulated* eye, the after-movement will take place, but more faintly than if the stimulated eye had itself seen the projection ground. When the after-movement is projected upon the printed sheet, *no after-image of the aperture* is present, and yet a slow drifting after-movement is seen. The fading of after-images will not account for the presence of the after-movement under these circumstances, but the *lack of body* in the after-effect may be accredited to the lack of the fading after-images in the unstimulated eye. Other instances will be noted later in this paper where the apparent movement takes place without the after-images, yet lacks the body of the normal after-movement.

This experiment was performed upon four subjects.

2. If black strips $\frac{1}{2}$ in. wide are pasted across the aperture so that they run obliquely from the upper left to lower right, a rotation of the drum will result in an apparent real movement of the black lines in an oblique direction. The after-effect is also in an oblique direction. It is the apparent direction of the real movement and not the actual direction which determines the after-movement. This is explicable on the basis of the after-image theory. If the retinal stimulation of lines moving vertically downwards plus association factors due to the oblique lines can produce the impression of oblique real movement, then the fading of after-images downwards plus the same association factors can produce the impression of after-movement upwards.

The after-effect of stroboscopic movement can be accounted for on this basis.

This experiment was performed upon two subjects.

3. Experimentation upon five subjects clearly establishes the following results: (*a*) If the subject observes the moving drum and then turns his eyes upon a printed page, the after-movement which is seen may often appear before the after-image of the aperture and will always disappear before the after-image. The after-movement reaches its maximal clearness before the after-image reaches a corresponding stage. (*b*) A faint after-movement may appear on the printed page when no after-image is secured. These two facts indicate a lower threshold for the after-movement than for the after-image. They are what one would expect from the after-image theory and the known facility in the apprehension of movement. Further one would expect the relative difference in fatigue, *i. e.*, the fading of the after-images, to be overcome before the absolute fatigue, *i. e.*, the existence of the after-image of the area as a whole. No exact quantitative determinations have been made.

4. A large series of tests were made on Plateau's spiral. Here we would expect the causal factor to be predominately retinal in as much as the movement went in all directions. Other factors may and do enter in as I shall indicate in the discussion of association and eye-muscle strain. In the present connection, I wish to stress one point merely, viz., if the subject observes the rotating spiral with one eye and then turns his other unstimulated eye to a figured surface, *he does not see an after-movement.* The same *negative* result is secured if the subject maintains his fixation on the disc. This is a matter of great importance because of its fundamental bearing upon retinal factors and because it contradicts the observations of earlier writers, notably Szily. Four subjects in all were used and the greatest care taken to secure reliable data. The subjects — the present writer excepted — were ignorant of the theoretical bearing of this test. There was therefore no reason why factors of associative control should dominate here any more than in the similar monocular tests

with the rotating drum where positive results were obtained. Some of the subjects noted an uneasiness of the field of projection as soon as they turned the unstimulated eye in that direction. Some thought of this as an after-movement. When these same subjects, however, were familiarized with the 'uneasiness' of the projection field which is *normally* and inevitably incident to shifting fixation from one eye to the other, they one and all denied having seen an after-movement. Other subjects were familiarized with the apparent shifting of the characters on the projection field due to shifting fixation before the regular tests were started. These subjects never claimed to see an after-movement. Previous experience had acquainted them with the nature of the normal after-movement as seen with one eye. There is one other precaution which is of great importance here. Before the subject turns his unstimulated eye toward the projection field, he must be certain to cover the other eye. This is obviously fundamental. As trivial as it may seem, it was a source of error in several of the present tests. I attribute the after-movement of the spiral which other investigators have seen with an unstimulated eye to a neglect of the above factors. That they are easily overlooked, my own experience has taught me.

The negative results here obtained give emphatic support to the retinal character of the after-movement secured from Plateau's spiral. This is to be contrasted with the positive results secured from the rotating drum under the same conditions.

Association Factors

By association factors I mean interpretative processes. As such they are an integral part of the after-image theory; because it is not sufficient that the after-images fade, this fading must be interpreted as a movement of the objects seen through the flux. This association factor seems to be a lineal descendent of the theories that explained the after-effect of visual motion in purely psychical terms. The representatives here are Budde, Lotze[1] and Zöllner. All hold in common to

[1] Lotze, R. H., 'Medicinische Psychologie,' Leipzig, 1852, S. 443–4.

the assumption that the after-effect is due to a predisposition of the mind to judge in a certain manner. Those who hold to a special movement center (*e. g.*, Exner, Szily and Wohlgemuth) are stating in neurological terms this persistent tendency to a certain interpretation. Why is the observer impelled to interpret stationary objects as moving in the opposite direction to the previously seen real movement? Lotze says it is because the mind has grown accustomed to seeing movement so that the habit now persists—a case purely of contrast. Wohlgemuth says the after-effect occurs because the direction of fatigue has been changed in certain central summation cells. One may be viewed as the neural counterpart of the other, because even a 'psychical' explanation must be neurologically conditioned. Wohlgemuth might claim that there was no conscious representative of the central movement processes per se,—but then Lotze does not, I think, maintain that the 'tendency' is overtly conscious. In either case it is only the end process, the after-movement, which is detected in consciousness.

My criticism of the after-image theory and the share that it must allot to associative or interpretative factors is that it does not go far enough. These latter factors may even *control* and *overcome* the former and any other causal factors that may be effective in the production of the after-effect. The most important facts in the present paper were discovered as a result of training the subject to *control the after-movement.* Strange as it may seem, no such tests have been brought to light in the literature of the subject. Experiments have been made to discover whether or not the after-movement would take place when the subject's attention was distracted, but that is as far as such tests have extended.

The following statements will indicate the essential facts bearing upon association factors:

1. *Voluntary Control of the After-movement.*—Five subjects were used in this test with from 50–200 trials per individual. Control of the present illusion is not so much a matter of long training as it is that of catching the proper method. The illusion is very insistent. One would expect this if retinal

factors are involved. After the first two or three trials when the subjects all had confessed their failure, the experimenter instructed them as follows: "You know the lines are not really moving. Why let yourself be deluded into the contrary belief? Set all of your will power against the after-movement. Notice that the lines next the fixation point never get farther away (or nearer)." These instructions, within 5 trials at least, brought success in this sense, the lines would stop and then flow, stop again and then flow on. The subject was then requested to stop the *two lines nearest the fixation* point and so to attempt gradually to extend his control over the whole area. In doing this great care must be used to maintain fixation and merely to attend to the lines in peripheral vision. Great stress was laid upon this point and the subjects exerted themselves to the utmost. If the fixation had shifted there would be excellent grounds for attributing this supposed control of the striped area nearest the fixation point to the fact that the area now fell upon a part of the retina which had been unstimulated by the real movement. Occasionally such shifts did occur. They could always be detected, however, by the appearance of a bright after-image of the aperture on the opposite side of the screen from the fixation. The subjects were instructed in this method of detecting a change of fixation. The observed area was small enough and distant enough to render such observation feasible.

By means of the simple method outlined above, all of the subjects were enabled to control from $\frac{1}{4}$ to $\frac{1}{2}$ of the drum area. This control tended to be intermittent at first, but became steady and continuous as the tests continued. The remaining $\frac{3}{4}$ or $\frac{1}{2}$ of the area was involved in the normal after-movement during the control. No subject succeeded in stopping the entire after-effect by the method here described. What control there was, I am inclined to attribute to association factors. Another possibility will present itself in the discussion of eye-muscle strain.

In order to secure a better control, the subjects were instructed to clench their fists and jaws and to secure muscu-

lar rigidity in general. Their heads were held upright during this procedure, so that there was no conscious eye-muscle strain. Under these conditions some of the subjects were able to extend the area controlled even to frequent complete success. This may also be termed control by association factors. General muscular rigidity or the feeling of bodily inertia apparently aided in bringing about the interpretation that some or all of the lines were really not moving at all.

The evidence for the effective presence of general association factors is not confined to experiments on control and to the general statement that fading after-images (or other retinal processes) must be interpreted. The following experiment and observations must also be considered:

2. The subjects (four in number) were each requested to fixate one of the dots during the real movement and then, when the drum was stopped, to turn their eyes to the central stationary line. This resulted in seeing a portion of the drum with a part of the retina previously unstimulated by the movement. One subject saw this portion of the drum (*i. e.*, the upper part, if the original fixation had been the upper dot) as stationary. The rest of the drum gave the characteristic after-movement. All of the other subjects saw two movements on the drum. These two either met or parted in the middle of the drum, *i. e.*, at the point of fixation, depending upon whether the real movement had been up or down respectively. One of these movements can be accounted for by the after-image theory; but the other opposing movement which corresponded to a part of the retina unstimulated by the original real movement must be explained in terms of association factors somewhat as follows: Where object *A* really moves towards *B* (really stationary) under conditions which do not favor a comparison with other objects in the environment, the phenomenon may be interpreted in three ways: (*a*) either *B* is stationary and *A* moves towards (or away from) it; or *A* is stationary and *B* moves with respect to it; or (*c*) both *A* and *B* move. It is this latter interpretation which was prevalent with the subjects of the present test. The theory of fading after-

images cannot explain this. Great stress should be laid upon the *narrowing of the field of attention* which introspection reveals as occurring with a steady observation of moving lines and spirals. The subjects become so engrossed with the mere movement or with the depth illusions involved that all else drops to a very low attention level. It is this fact which gives so much free play to factors of association and eye-muscle strain as we shall see below. Under every day conditions, the constant shifting of the visual field and of the interests tends to obliterate or check up the illusory data. But where conditions all favor the perception of motion, motion is readily perceived. The movements of the eyes in every day life may quite readily be seen to carry along a swinging and swaying of the objects of the visual world. In many ways these are trite psychological sayings. They deserve restatement here because of the light they throw upon the possibility of associative factors being instrumental in the perception of visual after-motion.

3. There are two other observations made by the subjects during the course of the experimentation upon the rotating drum which have a direct bearing upon the present topic. (*a*) During the real movement as well as during the after-effect, the sides of the white screen surrounding the drum may seem to move in the opposite direction to the drum. This is a phenomenon which receives very little attention in the literature. (*b*) Particularly during the after-movement, if the fixation is the lower dot and the after-movement is downwards, the lower part of the screen has been seen to move steadily upwards at a considerable velocity. Some subjects have voluntarily commented upon these two happenings. Others have had to have their attention called to the possibilities. Usually the attention is so engrossed with the drum that any movements of the screen are ignored or unseen. (*c*) The stationary drum was often declared to be moving slowly. This declaration was never made unless the subject was familiar with the normal after-movement. This steady flow of the lines or their persistent tendency to flow is possibly to be explained largely in terms of association

factors. The phenomena of retinal irradiation and of various entoptic activities would not account for direction of movement, although they remain as a possible material for interpretation in terms of directive movement. Eye-muscle strains aroused by expectation may be the cause here as they are in other phenomena noted below.

Eye-Muscle Strain

In 1863 August Classen published an article, 'Ueber das Schlussverfahren des Sehacktes,' Rostock, wherein he accounted for the after-effect of visual motion through feelings of innervation. I have not had direct access to this paper. It is not cited by Szily and many other writers. For clearness and completeness, I reproduce Wohlgemuth's summary. "He [Classen] denies that the phenomenon is due to involuntary eye movements since he could not discover such by objective observation. He agrees with J. J. Oppel that it is not due to after-images but denies that it is caused by a process in the brain. He looks for an explanation of the phenomenon in the reflex tendency of the eyes to follow any movement and the innervation of the antagonistic eye-muscles to resist it. When the eye is turned to a stationary object, the increased innervation continues but, being no longer adequate to the visual experience, produces visual vertigo (Gesichtschwindel). As in paralysis of an eye-muscle, the vertigo is not caused by the feeling of tension but by the feeling of impulse to contraction, *i. e.*, the sense of innervation."[1] Heuse[2] criticized this theory because if the eyes are strained in one direction no after-movement is produced in the direction indicated by the theory. Wohlgemuth[2] has two objections to the theory: (1) It involves the dubious doctrine of feelings of innervation. (2) It will not account for the after-effects of motions that proceed in many simultaneous directions.

The objection by Heuse, I shall meet with experimental data bearing directly upon the question. With the first of

[1] Wohlgemuth, *op. cit.*, p. 6.

[2] Wohlgemuth, *op. cit.*, p. 96.

Wohlgemuth's criticisms, I am in hearty accord. It is undoubtedly due to this factor of feelings of innervation as well as to the innate attractiveness of the opposing theories of after-images and special movement centers that Classen's mode of attack has been so long ignored. Wohlgemuth's second criticism betrays the persistent fallacy of investigators in this field. Why should a factor in order to be rated as causal need to be present in all types of after-movement? Eye-muscle strain may well be the dominant factor when the illusion of after-movement is obtained under one set of conditions and some other factor may be dominant under other conditions. I agree with the critic in thinking that any such factor as Classen suggests must have a very slight influence in such cases as those indicated. Indeed the foregoing pages have contained data on the rotating spiral conclusively proving this point. This does not mean, however, that under other conditions eye-muscle strain may not be an important causal factor. (1) I think there can be little doubt that eye-muscle strain reported in the form of kinesthetic sensory impulses can occasion the perception of motion. The researches of Carr and Adams upon the auto-kinetic illusion clearly indicate this. Furthermore, (2) we have the general fact that in all cases, of simple real movement, such as that of parallel black lines, the strain produced in the eye-muscles due to the inhibited reflex tendency to follow moving lines would be in the *proper direction* to explain the resulting after-movement. These two points are important in giving a theoretical background of probability to the following experiments and observations.

1. The importance of eye-muscle strain was first noted in the present investigation through attempts at controlling the after-effect. The subject was instructed to fixate the upper dot and bend his head forward in such a manner as to cause a severe strain upon the upper eye-muscles. He was told to control the after-movement if possible. The vision of the drum was unobscured by the eye-brows. The drum was now rotated downward for 20 secs. Two general types of results were obtained: (1) The subject saw no after-movement.

(2) An after-movement was seen, but its duration was very brief. It was in a normal direction, *i. e.*, opposed to the real movement. The duration of the after-movement under conditions of normal fixation and a 20 secs. exposure varied between the extremes of 15 and 20 secs. for all of the five subjects tested. Under the conditions of this experiment, the duration of the after-effect, when present, varied between 2 and 8 secs. with an approximate average of 6. Of the various subjects, one always secured a short after-movement. Three others secured an occasional after-movement. Another subject never reported one. The following report of eight tests made upon a member of the second class will illustrate this point. No reliable time measurements could be made.

Strained Fixation of Upper Dot. Real Movement Up

Test 1. Didn't see much movement to stop.
Test 2. Ditto.
Test 3. Didn't see any movement. Sure of fixation.
Test 4. Ditto.
Test 5. Everything stopped instantly with the real movement.
Test 6. Ditto.
Test 7. Ditto, very little effort.
Test 8. Ditto. Slight sensation of screen and himself going upwards.

The introspection in the last tests is worthy of notice. Not only was the normal after-movement absent, but the whole apparatus and he himself as well tended to float upwards. This occurred rarely with *P*, but subject *T* reported the sensation continually during this type of test. This is the conventional auto-kinetic illusion, with the exception that it occurs in daylight illumination and for complex objects. I refer the inability of surrounding objects to check the illusion in this instance to the intense concentration of attention.

The same control of the after-movement was obtained where the lower dot was fixated with a strain on the upper eye-muscles and the drum rotated upwards. Whenever an after-effect was present, it went downwards for a brief interval. Where the drum rotated downwards and the upper eye-muscles were strained during the fixation, control was still possible although it was more difficult when judged by the number of times that short after-movements appeared.

Two subjects under conditions of eye-muscle strain were unable to secure an after-movement when they tried to do so after they had been trained to control the after-effect by this method. Their failure is to be accounted for undoubtedly upon the basis of this habit.

Two explanatory factors are necessary in order to account for the results of this experiment: association and eye-muscle strain. The main point of the control was to secure conditions under which the dominating strains would either blot out or over-balance any strains due to inhibited eye-movements. This can readily be seen to have succeeded save where the strain was in the same direction as that which we sought to counterbalance. Here the explanation lies in interpretative factors due to the *Aufgabe* and to the fact that a mere bulk or mass of strains may be unfavorable to the after-movement by providing explicit standards of reference which serve to check up the (illusory) interpretation of eye-muscle strain. It is not surprising that short after-effects occurred when it is borne in mind that fading after-images (or other retinal factors) were present. The astonishing fact is the control, *i. e.*, the absence, either total or nearly so, of the after-effect. I was inclined at first to interpret the short after-effects here in evidence as due to retinal factors and to conclude that the period from 6 secs. to 15 or 20 secs. was in general represented by the eye-muscle strain factor. Such an explanation, however, is not valid. The experiment on the spiral above cited which indicated that strains are negligible in that type of after-movement revealed also the fact that the duration of the after-movement of the spiral was the same as that for the rotating drum. It is impossible, then, within the bounds of the present data to sever temporally the effects of retinal and muscular factors. It seems quite probable that the effective phase of the one may, under the present conditions, be confined within the same temporal limits as the other. One would not appreciably outlast the other, but both together would give a more intense after-effect than either alone.

2. If the subject observes the rotating drum with one eye

and then closes that eye and observes the stationary drum with the other eye, he secures an after-movement which is the same as that normally secured by the stimulated eye save that it is fainter and of shorter duration. This is in harmony with the results obtained by other investigators. This faintness may be described as a 'lack of body.' Three subjects were used in this test. All were able to secure the after-movement with the unstimulated eye. They varied only in their experience of the duration of the after-movement. If the subject turned his unstimulated eye toward a printed sheet, he saw the after-effect there. It was observed in the vicinity of the point of fixation, but *no after-image of the aperture was secured.* The subjects were tested in order to ascertain whether they could secure an after-image in the unstimulated eye from a square of black paper. The results were always and absolutely negative. The present phenomenon then cannot be explained upon the basis of fading after-images. Historically the case has been used as an argument in favor of central causation. Such a theory is inadequate and unnecessary. If the contrary were true, the spiral should have given an after-movement in the unstimulated eye. The after-movement seen by the unstimulated eye on the drum may be explained in terms of eye-muscle strain. Fatigue of the muscles of one eye is paralleled by fatigue of those of the other eye. In the case of the rotating spiral where the phenomenon is one of fading after-images, one would not expect an after-movement from an unstimulated eye. With the rotating drum, the real movement is all in one direction so that an asymmetrical eye-muscle strain is possible. In as much as the after-movement from the unstimulated eye is weaker than that from the stimulated eye, the present case offers further confirmation of the causal effectiveness of retinal factors.

There are certain other historical phenomena which have been interpreted as giving a basis for a central theory of causation. These, however, permit of a statement in terms of the harmonious action of the eye-muscles. I quote the following from Szily:

"Als schräges Liniensystem dient der zu meinem Kontrastversuch (S. 123) benützte gestreifte Kattun, mit welchem man den daselbst ebenfalls erwähnten grossem Rahmen in der gewünschten Richtung überspannt. Dieses beliebig schräge Liniensystem wird hinter einem Schirm mit einem Ausschnitt von 20 cm. Durchmesser langsam in horizontaler Richtung vorbeigeschoben, indem der Beobachter mit einem horizontal umkehrenden Prisma vor einem Auge eine genau in der Mitte des Ausschnittes angebrachte Fixationsmarke zu binokularer Vereinigung bringt, wodurch auch die beiden bewegten Flächenbilder genau übereinander gebracht werden (26). (Ich ziehe diese Anordnung der Benützung einer schräg linierten Kymiongraphion-trommel vor, weil bei dieser, infolge der Konvexität der Fläche, die äussersten seitlichen Teile derselben keine ganz entsprechende binokulare Deckung erfahren wurden.) Auf die angegebene Art erhalten die beiden Augen gleichzeitig symmetrisch entgegengesetzte schräge Bewegungseindrücke, bei welchen sich die Erscheinungen des binokularen Wettstreites in ausgiebigem Masse geltend machen. Wendet man nach genügender Einwirkung die Augen plötzlich nach dem Projektionsgrund, so gewahrt man ein durchaus *vertikales* Bewegungsnachbild (Bewegungsrichtung nach *oben*, wenn die Verschiebung der Tafel in die Richtung der Neigung der Konturen stattgefunden hat; nach unten im entgegengesetzten Falle). Schliesst man hingegen sofort nach Empfang des objectiven Eindruckes plötzlich ein Auge, so verläuft das nun wahrgenommene Nachbild, entsprechend dem diesem Auge allein zuteil gewordenen Eindruck, *schräg*, jedoch zweifellos *nicht in dem Masse schräg*, als wenn der das Nachbild auslösende Eindruch während einer gleichen Zeitdauer dem einen Auge allein, ohne gleichzeitige symmetrisch entgegengesetzte Erregung des zweiten zugeführt wird."[1]

This phenomenon described by Szily is what would be expected if eye-muscle strain were the important factor. The tendency is for the eyes to move in divergent directions. This results for the left eye, *e. g.*, in a strain which is not

[1] *Op. cit.*, S. 129–130.

directly opposed to the oblique movement seen by that eye, but is modified toward the vertical by the strain on the other eye. I have not repeated this experiment.

Szily also reports an experiment upon rotating spirals.[1] The spirals were opposed in direction of movement. One was viewed by one eye, and the other by the other eye. No after-movement resulted.

I have repeated this experiment carefully upon myself. Positive results were secured, *i. e.*, the superposition of the spirals did not destroy the after-movement. Two spirals (black upon white discs) which were wound in contrary directions were placed upon a color mixer having two spindles side by side. Both spindles were run from the same belt by a motor. A glass prism was used to superimpose one disc upon the other. The subject sat at an approximate distance of 1½ feet. This enabled him to secure a clear image of each disc. Tests were made in which the outward turning disc was superimposed by the right eye upon the inward turning disc seen by the left eye. Tests were also made in which the discs were interchanged, and also some in which the prism was held before the left eye. These varying conditions had no detectable effects upon the results. The interval of exposure was 20 secs. (time counted with a metronome). In one series a period of 40 secs. was used. The experimenter found no difficulty in superimposing the discs by keeping the center nuts co-incident.

During the real movement, the subject never felt that he secured a mixture of the movements. Retinal rivalry was present. The after-effect was projected upon a large dull white sheet of paper covered with printed characters and distant 5 feet from the observer.

Results.—An after-movement was present in 95 per cent. of the trials. It was practically always an outward movement. A very few times, it could only be characterized as movement. Retinal rivalry was not noticed. The after-effect was fainter than if both eyes had seen the same real movement. The after-movement present was as positive

[1] *Op. cit.*, S. 128–129.

and as genuine as an after-movement could be. It was not a mere uneasiness such as is normally incident to a shift of fixation. Its duration averaged around 6 secs. or more. The domination of the outward after-movement is probably based upon a greater facility for catching the attention. This has not been thoroughly tested, but scattered results would suggest its truth.

The results here secured are in harmony with the theories of retinal and associative causation. Where corresponding retinal points are stimulated by fading after-images in such a manner as to elicit contrary judgments, one can expect either rivalry or a domination of one judgment. It is indeed conceivable that the two retinal processes should so nearly balance, *i. e.*, be so nearly of the same intensity, as not to lead to a judgment of movement at all. The case here is different from that of a so-called binocular mixture of color. There one deals with a central fusion of sensory qualities. Here the process is essentially judgmental in that it is an interpretation o one phenomenon (fading after-images) as a movement of another (stationary objects) totally irrespective of sensory qualities. (Obviously I am not holding that this interpretation takes place as a process overtly conscious.) If there is not enough of the fading after-images in one direction dominant—due either to different relative intensities or to varying powers of attracting attention—the judgment or interpretation of movement will not be aroused. Central processes are indeed involved, but not in the sense of special movement centers.

One other case may be drawn from the literature. Wohlgemuth[1] describes tests in which by successive real movements of opposite sign, he so fatigued the subject that no after-movement, or only a faint one, resulted. When the real movement was at right angles to the ones for which fatigue had been set up, the after-movement was unaffected. Here again one would find difficulty in accounting for the phenomenon by the after-image theory. It is intelligible, if eye-muscle strain is a causal factor. One would not expect a

[1] *Op. cit.*, pp. 78–80.

fatigue of the superior and inferior eye-muscles to fatigue the internal and external ones. This is assuming the validity of Wohlgemuth's observations.

3. The *stationary* drum may be made to appear to rotate by straining suitable eye-muscles. In this experiment the seven subjects used were requested to observe one of the fixation points and to describe any movement seen. Four eye positions were used: (*a*) with head tilted back, giving strain on the lower eye-muscles; (*b*) with head bent forward, but with eyes turned so as to look toward the side, giving strain upwards upon lateral muscles (right and left sides were used indifferently); (*c*) with head bent forward, giving strain upon the upper muscles; (*d*) with head bent back, but with eyes turned to the side, giving strain downwards upon lateral muscles. The results were consistent for all subjects. The lines upon the drum drifted in the direction of the eye-strain either upwards or downwards within a few seconds after the fixations were secured. The best results, *i. e.*, the clearest 'after-movements,' were obtained from the strain of the lateral muscles. The lower muscles were next; and the upper muscles, last in point of efficiency. The lesser efficiency of these two classes lay in the irregularity of the appearance of the movement and not in the velocity and clearness of the movement when it did appear. This inefficiency is not surprising when it is remembered that the production of apparent movement by eye-muscle strain is based upon interpretations of the strains under unusual conditions. The forward bending of the head with the eyes looking straight in front is a far more usual type of eye position than the others, *i. e.*, it offers more secondary cues for the counterbalancing of the apparent movement. After the subjects had been tested by these methods, it was not necessary to maintain extreme peripheral fixation in order to secure the 'after-movement.' The effect could be produced by slight inclinations of the eyes in the above mentioned directions.

In the light of the discussion earlier in the paper, it is more reasonable to interpret the present results on the basis of eye-muscle strain than upon the basis of any slight nystag-

mus present in cases of peripheral fixation. The results with slight strains are also antagonistic to such an interpretation. The results of this experiment refute the objection attributed above to Heuse that eye-muscle strains could not produce movement of a stationary drum. (See p. 264 above.)

4. The problem here was to determine whether eye movements during the observation of the real movement would inhibit the after-effect. In the literature, there is much dispute whether or not eye movements are favorable to the perception of the after-effect. J. J. Oppel thought fixation favorable. He is followed in this by Szily, Exner, Wohlgemuth and others. Helmholtz maintains that where the fixation is rigid no after-movement takes place. He accounts for Oppel's results on the basis of poor fixation. Both of these observations can be understood from the point of view of the present paper. A constant fixation, if rigidly maintained, is a most favorable condition for the appearance of association factors which tend to control the after-movement. Eye movements, if rapid, may accomplish two things: (1) They may prevent the appearance of an eye-muscle strain in any one direction. (2) They may weaken the after-image by not subjecting the same retinal area to constant stimulation. In either case the after-effect may be absent or extremely faint. In the test to be cited immediately, the latter factor was only faintly if at all operative. However the tendency to associative control must be recognized as well as the absence of eye-muscle strain.

In the present experiment, the three subjects used were instructed to move their eyes *rapidly* over the *central* area of the moving drum. They all accompanied this with head movements and general muscular activity. At the end of 20 secs., the drum was stopped and the subject either fixated the upper dot or turned his eyes to a printed page. In neither case did an after-movement occur, although with a steady fixation and 5 secs. rotation of the drum, a good but short after-movement is seen. The subject who was most thoroughly tested was able to secure a faint after-image of the aperture within the 20 secs. of real movement, but no after-

movement appeared. This is of particular interest in view of the results above given which indicate that the after-movement normally has a lower threshold than the after-image.

5. This is another experiment in which I have tried to analyze out the influence of the factor of eye-muscle strain. A mirror was placed below the rotating drum in such a position that the subject could not see between the two.[1] A tack or other small object was placed at the boundary between drum and mirror for a fixation point. Whenever the drum rotates, the reflection in the mirror moves in the opposite direction. Three subjects were used. During the observations the eyes were from one to two feet from the fixation point.

The normal after-movement secured moved in two different directions corresponding but opposed to the directions of the real movement. Two methods of control were used, strain on the upper eye-muscles and mere voluntary effort without conscious strain. All three subjects were able to secure a partial control over a part of one after-movement by this latter method. In other words, part of the drum area or part of the mirror area would be seen as stationary, while the rest moved in the accustomed manner. At some time, each subject was capable of stopping one movement entirely. This control was apt to be brief and to be followed by the regular phenomenon. Quite as frequently, the subject would set up an alternation of after-effects, *i. e.*, a movement would be seen on the drum and then one on the mirror, etc. The subjects felt that they could see the after-movement in either locality as they chose. One subject was able to control all after-movement at all times. Indeed at the beginning of the tests, it was impossible for him to secure any after-effects at all. In as much as he was the most practised observer of all, this can hardly be attributed to poor introspection. It must rather be put to the credit of associative control.

[1] See Julius Hoppe, 'Studie zur Erklärung gewisser Scheinbewegungen.,' 1894, *Ztsch. f. Psych. u. Physiol.*, Bd. 7, S. 31.

When control was attempted with eye-muscle strain, the same general phenomena occurred which have just been described. The following qualification, however, should be made: It was a rare occurrence when an after-movement appeared in a direction opposed to the strain (the upper eye-muscles alone were used). It is possible that this was due to suggestion independently of the strains.

6. Two general observations may be cited here in support of the presence of a factor of eye-muscle strain. (1) At intervals with two subjects the entire apparatus seemed to float upwards along with the after-movement. At such a time there was no apparent movement of the drum within the general movement of the whole. There was no doubt in the subject's mind concerning this occurrence. It was a clear and irresistible feeling. This general movement is clearly in a class with the auto-kinetic illusion. That it should take place so clearly in daylight illumination is not surprising. The conditions of observation favor extreme concentration of attention which minimizes any corrective influence of surrounding objects. (2) The after-effect of motion is determined by the apparent direction of the real movement. This has already been discussed (see above, p. 257) in relation to the after-image theory. It remains to point out here that eye-muscle strains may also be regarded as a possible factor. The eyes tend to follow the real motion in the direction that it *seems* to be taking and not in the direction which it really has objectively. Introspection verifies this. The resulting strain will, therefore, be opposed to this apparent movement and will aid in the production of the after-effect.

7. On page 257 of this paper, I spoke of having data which suggested the possible influence of some so-called 'mysterious factors' in the production of the after-movement. Those data were obtained from a preliminary study of the visual after-effects to be derived from he use of a stimulus which occupied most or all of the visual field. A full treatment of this problem is reserved for another paper. At this time a few facts only need be given. Szily[1] found that visual

[1] *Op. cit.*, S. 122–126.

movement of large extent was followed by an after-movement *in the same direction.* His method was to project the after-movement upon objects in the room. Wohlgemuth[1] attempted to repeat the test, but did not project the after-movement upon neighboring objects. He stopped the moving curtain and watched for the after-effect upon this. His results were almost entirely negative, *i. e.*, he and his observers got either no after-movement at all or at times a slight one in the normal direction.

In performing this experiment, I have used a large dark blue and white striped cloth (stripes $\frac{5}{8}$ in. wide) looped about two rollers. The total area exposed was 6 ft. long by 4 ft. high. Rotation was produced by a motor. Most of the observations have been made upon the writer himself. They have been checked up, however, by data from two other observers. At present the results are incomplete. Of one fact, however, there can be no doubt whatsoever, viz., after a 20–30 sec. stimulation, if the eyes are turned to the wall of the room, this seems to move in the same direction as the rotating lines. Particularly when the after-movement has been projected upon a printed sheet, I have gotten another simultaneous phenomenon. The point of fixation has moved in a direction opposite to the real movement. It is as though a shadow movement were passing in the same direction as the real movement and were causing the interpretation of an oppositely directed movement in the fixation point. Often this latter movement is late in making its appearance. In any case, the after-effect is not sharply localized, but seems to be concentrated in the central part of the field of vision.

Szily accounts for the phenomenon on the basis of contrast with the normal after-movement which he always sees in the periphery. The contrast is based upon a difference of threshold for movement. I am desirous of securing more data before accepting or rejecting this interpretation.

Summary and Conclusions

1. Visual after-movement is a complex phenomenon and its causes will not admit of reduction to unity.

[1] *Op. cit.*, p. 72.

2. The factors in its production are: retinal changes (probably fading after-images), association factors and strains in the ocular muscles. Either of these three may dominate over the others. Retinal changes may produce an after-effect even though this is opposed to the strains and association factors then active. Eye-muscle strain may inhibit any after-effect although the retinal factors and possibly the association factors are opposed to it. The same is true *mutatis mutandum* of the interpretative or associative factors.

3. The after-movement from a system of parallel lines is caused by all three of the factors above mentioned.

4. The after-movement from a rotating spiral is predominately of a retinal origin.

5. Asymmetrical eye-muscle strains (to which an effective nystagmus would reduce) may cause apparent movement of a stationary system of parallel lines. If the real movement of the drum is observed with one eye only and the after-effect is sought for with the other eye only, the search is successful. This can be accounted for on the basis of the harmonious action of the eye-muscles. A similar explanation holds for most cases of binocular fusion of the after-effects.

6. Stimulation of the eyes by movement (parallel lines) of large extent gives anomalous results. Two movements are seen, one going in the *same* direction as the real movement and one in the *opposite* direction. The explanation of these after-effects is still to be sought.

A COMPARISON OF THE ORDER OF MERIT METHOD AND THE METHOD OF PAIRED COMPARISONS

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The order of merit method as we know it to-day was first employed by Professor Cattell in his study of 200 shades of gray. This method consists in the arrangement of the specimens of the series when all of the specimens are presented to the subject as a series. The paired comparisons method involves comparing every specimen in the group directly and separately with every other specimen, the relative order in the final series depending on the number of preferences given to each specimen. This method of course takes longer and is more tedious both to operator and to subject than is the order of merit method.

The present experiment is an attempt to compare these two branches of the serial method in their statistical applications for the sake of determining their relative ease, reliability and consistency. Such a comparison is important in view of the fact that the one method is vastly to be preferred to the other in convenience of its operation.

The materials used in this experiment were of three kinds, selected with a view to a comparison of the two methods in their application to three types of judgment, viz., very objective judgment, semi-subjective judgment, and very subjective judgment.

Weights were chosen as material presenting an objective order definitely and mathematically prescribed. The series used included 15 weights identical in outward appearance but varying as follows: increasing by uniform increments of 4 per cent.; 100 gr., 104, 108.2, 112.5, 116.9, 121.7, 126.5, 131.6, 136.9, 142.3, 148.0, 153.9, 160.1, 166.5, 173.6 gr.

As a material involving semi-subjective judgments the

specimens of handwriting were used which are presented by Thorndike in *Teachers College Record*, Vol. XI These specimens are graded in a scale of excellence as determined by the average opinion of competent judges in general. This scale is a semi-subjective one, in that it depends solely upon the average opinion of competent judges and cannot be referred to physical scales as weights can. The Thorndike scale is the result of some 20,000 ratings and represents measurements far more accurate than any one could make without it. The specimens used in this experiment are those numbered 18, 17, 16, 15, 14, 13, 12, 11, 10, 9, 8, 7, 6, 5, 4, in the scale.

The material used for a comparison of the two methods in very subjective judgments was a series of propositions to be ranged by the subject according to the degree of belief in the fact stated by the proposition. There is no objective scale whatsoever for the measurement of belief in these propositions as there is for the measurement of weights and excellence of handwriting. The criterion of accuracy in this type of judgment is, in this case, simply the average arrangement by the individuals of the given group.

The propositions in their order of belief according to the subjects passing judgment in this experiment, are as follows, by the order of merit method. (The figures in parentheses at the left indicate the position of the given proposition by the method of paired comparisons.)

1. 2 + 2 = 4.
2. There exists an all-wise Creator of the world.
3. Geo. Washington was a real person.
(5) 4. Music more nearly approaches the divine than does poetry.
(4) 5. Vergil wrote the Æneid.
(7) 6. Man has evolved from one-celled protoplasm.
(6) 7. The most honest man I know will be honest ten years from now.
8. Dark-haired men are handsomer than light-haired ones.
(11) 9. Death ends individual existence.
(9) 10. The moon is larger than Jupiter.
(12) 11. It never rains but it pours.
(10) 12. It will rain next 4th of July.
13. Only the good die young.
14. Opals are unlucky.
15. 2 + 2 = 7.

The subjects in this experiment were students in Barnard

College in the regular laboratory course in experimental psychology. For each type of judgment investigated ten subjects arranged the specimens by both methods. The ten were divided into two groups; five of them arranging the specimens first by the O. M. method and one month later by the method P. C., and the other five arranging them first by the P. C. method and one month later by the O. M. method. This plan eliminated any error which might arise through one method assuming a superiority over the other by virtue of the precedence of one or the other in order of presentation. This plan also offers an opportunity for an investigation of the problem suggested by Lilien J. Martin, as to whether one of the methods under consideration must be supplemented by the other if the best results are to be obtained (14). Although the method to which she refers by the name of 'the method of constant differences' does not correspond exactly to the method of paired comparisons as it has been used in this experiment her proposal to supplement one of these methods by the other suggests an interesting incidental problem in the present attempt to compare the relative merits of the two methods. The fact that in one group of subjects the series was arranged first by O. M. and later by P. C.—and that in the other group this order was reversed, offers the basis for an observation of the reciprocal effects of the two methods.

The method of paired comparisons, a long and tedious one, was reduced in this experiment to its lowest possible terms by a special system, which enabled the experimenter to present the pairs to all of the subjects at the same time—one subject being presented with one pair while another subject was presented with another pair, etc. This was accomplished by a modification of the system of exhibiting the pairs which was used by Cohn. The latter consists in a presentation in the following order: 1–2, 3–1, 2–3, 4–2, 3–4, 5–3, etc., each pair involving as one of its numbers one of the specimens used in the preceding pair—and one to be used in the pair immediately following. According to this system, if the pairs are to be exhibited say to 5 or 10 subjects, the second pair, involving

one of the specimens belonging to the first pair, could not be exhibited until the first pair had been judged by all the subjects and had been returned to the operator. In order to be able to have several pairs in circulation among the subjects simultaneously the series of combinations was rearranged in groups which included in successive pairs only those specimens not already upon exhibition in another pair of the group. For instance if the pairs 1–2, 3–4, 5–6, etc., be exhibited, seven pairs could be in circulation simultaneously in regular order, so that the subjects having been provided with a plan of the order of presentation, could indicate as each pair came to them their preference for one specimen or the other. Thus the weights having been numbered at random were exhibited in pairs, each subject after having passed her judgment on each pair as it came—passing that pair to the next subject, and receiving, on the other hand, the pair just judged by the subject preceding. The operator receives the specimens as they come from the last subject in the group—and selects, according to the fixed plan, the new combinations which are to enter circulation. The subject simply writes, after each number on the outline with which she is provided, the number of the weight preferred—or simply R or L according as she prefers the specimen on the right or left. The specimens of handwriting and the belief propositions were pasted on separate pieces of cardboard—and the combinations inserted into heavy cardboard frames which were passed from one subject to another.

In this way 105 combinations of the 15 specimens of the given series were judged by five subjects in about 50 minutes for weights—in about 40 minutes for handwriting—and from 40–50 minutes for beliefs. The time required for the judgment of a series by the O. M. method will average about 5–8 minutes for each person, varying with the material to be judged. If duplicate series are available, as they are with beliefs and handwriting for example—the series could be presented to the whole group in the time required for one person to make the judgment.

In working up the results obtained by the two methods,

the demand upon the time and energy of the experimenter is increased many fold in the case of the method of paired comparisons. The number of preferences for each of the 15 specimens must be counted up for each subject, and the serial order determined according to the number of preferences given. This requires from 15–30 minutes for each subject's judgment of any given series. In the O. M. method the serial order is already determined by the judgments themselves, and no further computation is necessary.

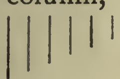
The time required for judging the series of weights in this experiment was longer than that required for handwriting because of the fact that the weights represented differences just perceptible, whereas the specimens of handwriting represent differences much more easily discerned. An interesting problem would be to work out, for different sorts of materials, series in which the difference between the specimens for any given material would be psychologically equal to the differences between the corresponding specimens of any other material.

The results obtained in this experiment can not be used in a strict comparison of the two *materials*, weights and handwriting but are intended mainly as a basis for comparison of the two *methods* used, and the materials may vary in many ways.

Procedure.—In the first month, November, 1912, the series of beliefs and the series of handwriting specimens were presented to five subjects separately with the following written instructions. For beliefs: "Arrange these propositions in an order of merit according to the degree of your belief in them. Place at the top the proposition in which you believe most firmly. Place next, the second in the order of belief and so on until the series is complete, with the proposition in which you believe least of all at the end of the series. (Please do not discuss the experiment with other members of the class.)" For handwriting: "Arrange these specimens of handwriting in an order of merit with respect to their excellence. Place at the top of the series the specimen which you judge to be the best. Place next to this, the specimen which you judge

to be next best and so on until all have been placed in order with the one you judge the poorest at the bottom of the series. (Please do not discuss the experiment with other members of the class for the time being.)"

In the same month, five other subjects judged the given specimens of handwriting and of beliefs by paired comparisons, according to the plan described above. The instructions, understood by each subject before the exhibition of the pairs, were as follows: For beliefs: "I shall present to you, two by two, a series of propositions. As each pair comes to you, decide in which of the two propositions you believe more firmly—and designate the one in which you believe more firmly by writing its number (I. or II.) in the space designated by the plan of presentations." Each pair exhibited was inserted into a cardboard frame with the numbers I. and II. by which to designate the two specimens. The plan of presentations was a sheet of paper for each subject on which columns of figures were arranged in order of the presentation of the various combinations, 27 in the first column, 23 in the next, 19, 15, 11, 7, 3, making the familiar

 plan of the paired comparisons order of presentation.

In the second month, December, 1912, the two groups were reversed; each doing what the other had done one month before.

In the third month, January, 1913, a new group of ten subjects was selected to judge the weights. Five of them made the judgment by order of merit under the following instructions: "Arrange these weights in an order of their heaviness, in a row on the table. Put the heaviest weight at the left end, the lightest weight at the right end, and the others ranged accordingly between them."

The other five made the judgment by paired comparisons with the following instructions: "I shall present to you, in pairs, a series of weights. As each pair comes to you, lift each weight successively with your right hand, decide which of the two is the heavier, and indicate the heavier weight by writing its number in the space designated by the plan of presentations." (The weights were numbered at random.)

In the fourth month, February, 1913, these two groups exchanged methods as the others had done.

The data obtained in these experiments offer material for the investigation of several problems connected with a comparison of the two methods.

Table I

Weights

Grams	Order of Merit			Paired Comparisons		
	Av.	Av. Order	Av. Variation	Av.	Av. Order	Av. Variation
173.6	1.1	1	.18	1.25	1	.35
166.5	2.5	2	1.00	1.85	2	.24
160.1	3.5	3	.90	3.05	3	.28
153.9	4.3	4	.76	4.20	4	.38
148.0	4.8	5	1.20	5.00	5	.50
142.3	5.6	6	1.04	6.00	6	.40
136.9	7.0	7	1.00	7.50	7	.85
131.6	7.8	8	.52	7.70	8	.96
126.5	9.0	9	.60	8.40	9	.66
121.7	10.5	10	1.20	10.80	11	.38
116.9	11.2	12	.68	10.50	10	.50
112.5	11.1	11	1.30	11.90	12	.49
108.2	12.9	13	.54	13.40	13	.58
104.0	13.9	14	.54	14.10	14	.54
100.0	14.8	15	.32	14.40	15	.68

Table II

Hand-Writing

No.	Order of Merit			Paired Comparisons		
	Av.	Av. Order	Av. Variation	Av.	Av. Order	Av. Variation
18	1.4	1	.64	1.10	1	.18
17	2.4	2	.68	2.55	2	.97
16	3.7	4	.82	3.32	3	.60
15	3.2	3	.92	4.42	4	1.34
14	4.9	5	.76	5.45	5	.86
13	6.1	6	.92	6.50	6	.96
12	6.7	7	.76	6.62	7	1.20
11	8.1	8	.36	7.85	8	.38
10	8.5	9	.80	8.87	9	1.08
9	10.0	10	0	9.85	10	.38
8	11.1	11	.18	11.30	11	.48
7	11.9	12	.18	11.80	12	.62
6	13.2	13	.32	13.10	13	.54
5	13.8	14	.32	13.90	14	.46
4	15.0	15	.00	14.85	15	.27

TABLE III

BELIEFS

No.	Order of Merit			Paired Comparisons		
	Av.	Av. Order	Av. Variation	Av.	Av. Order	Av. Variation
I.	1.1	1	.18	1.50	1	.40
II.	3.1	3	.36	3.25	3	.95
III.	2.9	2	1.64	2.90	2	1.86
IV.	5.6	4	1.52	5.25	4	1.00
V.	6.3	6	3.46	7.55	7	3.88
VI.	6.0	5	.90	5.30	5	1.66
VII.	7.1	7	1.10	6.60	6	1.80
VIII.	8.3	9	2.90	10.55	11	2.36
IX.	7.9	8	1.70	8.25	8	1.65
X.	11.2	12	1.08	10.15	10	1.25
XI.	10.5	10	2.60	9.40	9	3.00
XII.	10.6	11	1.28	10.95	12	1.26
XIII.	12.7	14	1.36	12.55	14	1.64
XIV.	12.0	13	1.40	12.10	13	.94
XV.	14.7	15	2.97	14.25	15	.75

On the basis of the data indicated in the accompanying tables of results we may formulate the following problems, each of which involves a comparison of the two methods with reference to that problem.

I. The variability of each specimen from the average position accorded to that specimen—and the average variability of the series for each method and for each type of judgment.

II. The correlation of the average order with the objective order by each method in the case of weights and that of handwriting where the objective order is already determined.

III. The correlation which obtains between the arrangements made of any given series by the one method and the arrangements made of that series by the other method.

IV. The average correlation of the individual subjects with their group average—by the one method and by the other.

V. A correlation between the individual's correlation with the group by the one method and the same individual's correlation with the group by the other method. This will show whether an individual who is representative of his group by the O. M. method is also representative of his group by the P. C. method.

TABLE IV

Obs.	Individual Correlations with the Group: Order of Merit, r	Order of Merit, Variation	Paired Comparisons, r	Paired Comparisons, Variation	Variability of the Series: O.M.	Variability of the Series: P.C.	Correlation with Objective Order: O.M.	Correlation with Objective Order: P.C.	Correlation of Methods
WTS:									
Cr	.925	.037	.975	.010					
D	.982	.019	.964	.021					
F	.975	.012	.972	.013					
M	.983	.020	.986	.001					
W	.939	.024	.993	.008	.79	.52	.997	.997	.989
B	.939	.024	.993	.008					
Bu	.964	.001	.997	.012					
Ca	.947	.016	.983	.002					
G	.986	.023	.989	.004					
McC	.989	.026	.993	.007					
Av.	*.963*	*.020*	*.985*	*.009*					
HAND-WRITING:									
G	.993	.012	.988	.016					
H	.918	.063	.963	.009					
R	.997	.016	.991	.019					
R′	.986	.005	.981	.009	.51	.69	.997	1.000	.997
S	.972	.009	.902	.070					
B	.986	.005	.957	.015					
L	.979	.002	.975	.003					
Mc	.993	.012	.974	.002					
P	.989	.008	.997	.025					
Y	.997	.016	.988	.016					
Av.	*.981*	*.015*	*.972*	*.018*					
BELIEFS:									
G	.881	.012	.884	.025					
H	.881	.007	.935	.024					
R	.917	.048	.870	.011					
R′	.981	.107	.949	.090					
S	.918	.049	.880	.021	1.63	1.63	(No objective order)		.979
B	.790	.079	.780	.079					
L	.832	.037	.896	.037					
Mc	.943	.074	.903	.044					
P	.777	.092	.750	.109					
Y	.820	.049	.740	.119					
Av.	*.874*	*.055*	*.859*	*.056*					

VI. A comparison between an individual's correlation with the average in handwriting and the same individual's correlation with the average in beliefs—by the two methods.

VII. A comparison of the group who made their judgments first by the O. M. method with the group who made their judgments first by the other method. This involves a com-

parison of the two groups with reference to the possibility of the judgment by either method being improved by the fact that the other method preceded it.

IX. A conclusion based upon all of these comparisons, and upon other points of comparison indicated by the actual employment of the two methods in this experiment and in their historical development.

Discussion of Results

I. The average variability of the position of each weight from the position of that weight as determined by the average opinion, is, by the order of merit method, slightly greater than it is by the other method. With the handwriting the exact opposite of this is true. It is impossible to determine whether this difference is due to the difference in material or to the difference in the individuals arranging the material.

It is of course conceivable, taking these averages in isolation, that the P. C. method is particularly adapted to judgment of weights and the O. M. method to judgments of handwriting. This however is disproved by the exceedingly high correlation of the two methods with a given material.

It might possibly mean that the one method is particularly favorable to the one group, and the other method to the other—but a comparison of the variabilities in beliefs with those in handwriting shows that the group which performed both of these types of judgment does not consistently prefer the one method to the other at all. Averaging the average variations for the three types of judgment we find a difference of only .08 between the two methods.

These differences in variability then may be due to the materials themselves, apart from any influence of methods, or they may be due to the groups themselves, apart from any consideration of methods; but they are evidently not due to any influence of the methods themselves.

The relatively high degree of variability in the arrangement of the beliefs is an interesting index to the subjectivity of the material. The subjectivity of the judgments of handwriting as compared with those of weights is obscured prob-

ably by the fact noted above that the differences between successive weights were differences just perceptible, while those between successive specimens of handwriting were much more easily discernible. In other words, subjectivity of judgment may be due either to individual variation in the standards or to the smallness of the differences presented, and in the present case these two types of subjectivity are not isolated.[1]

II. In the judgment of weights the correlation of the average order as determined by the group arrangements, with the objective order as determined by the weights in grams, is exactly the same for the one method as it is for the other. In the judgments of handwriting the correlation with the objective order is almost identical for the two methods—representing a difference of only .003 in favor of the P. C. method. The correlations between average order and objective order are practically identical for the two methods.

III. It may be seen from the table of correlation results that the order obtained by means of one method in a given material is almost identical with the order obtained in the same material by the other method. This would seem to indicate that the two methods are interchangeable from the standpoint of direct results alone as well as from the standpoint of variabilities and correlations with the objective order.

The correlation between the two methods does seem to vary slightly with the material judged. There is a possibility that subjectivity of judgment if it were greatly increased might involve a variability in the results as obtained by the one method or the other. In the present experiment however the correlation between the two methods even for beliefs is almost .98.

The average correlation between the two methods for the three types of judgment is .987. This indicates that it were very unnecessary to employ either of these methods which for any reason is less to be preferred than the other—if the purpose is to obtain merely general results.

[1] *Cf.* Hollingworth, 'Experimental Studies in Judgment,' Ch. X.

IV. The individuals of the group correlate with their average almost exactly as well in one method as in the other. The differences in the averages of the individual correlations with their group in the two methods lie in every case within the limits of the probable error of those correlations. This means that the two methods are equally efficient if we consider their results from the point of view of individual differences in variability from the group average. The individuals on the whole depart from their group no more in one method than in the other.

V. The *individual's* correlations with the group average are so nearly identical in the two methods that a comparison can hardly be made between an individual's standing in relation to his group in one method and his standing in relation to his group in the other method. Where these differences are large enough to be considered there is a fairly high correlation between the two methods in this respect. An individual who represents his group in O. M. also tends to represent his group in P. C. This correlation in both handwriting and beliefs is +.70. In weights the relation between the two methods in this respect is almost a random one (r = +.01). The cause for this is not apparent unless it is due to the fact that the individual differences in correlation by P. C. are so very insignificant as to make the order of correlations subject to chance and very unreliable. (The average variation among these is only .009.)

The judgments in handwriting and beliefs would indicate a tendency for the individual who is representative of his group in the one method to be also representative of his group in the other method.

VI. Having determined whether an individual representative *in one method* is representative also in the other, it is interesting to determine whether an individual representative of his group *in one type of judgment* is also representative in another type of judgment. This latter determination must be based upon a comparison of the judgments of handwriting and of belief only, since the weights were judged by an entirely different group of subjects.

By the O. M. method there is simply no relation at all between a person's judicial capacity in handwriting and the same person's judicial capacity in beliefs. The correlation between the two orders of the individuals in the group, the one as they stand in handwriting judgments, the other as they stand in judgments of beliefs, is expressed by the figure .01, a zero correlation.

By the method of P. C. this correlation is increased in a negative direction to −.35 although there is no apparent reason why, in P. C., there should be a tendency for an individual who is a good judge of handwriting to be on that account a poor judge of the validity of propositions. The negative correlation in this instance remains a mystery—and the fact that this correlation varies from a corresponding correlation obtained by the other method offers the first and only discrepancy in the equal efficiency of the two methods.

VII. Does a given method tend to give better results when it has been preceded by the other method than it does when used alone?

In order to answer this question we may compare the average correlation of the individuals of one group with the average correlations if the individuals of the other group, through the use of the one method and of the other. The following table shows these average correlations for the two groups—by the two methods and for each type of judgment. The group in which the P. C. method preceded the O. M. method, we may call the P. C. group. The group in which O. M. method preceded the P. C. method, we may call the O. M. group.

The averages are as follows:

	P.C. Group		O.M. Group	
	P.C.	O.M.	O.M.	P.C.
Wts.	.987	.961	.965	.990
H.W.	.965	.973	.989	.978
Beliefs	.903	.906	.832	.814
Av.	.949	.947	.929	.927

If we assume that the method which is the second to be performed tends, on that account, to give better results than the one first performed, then the figures in the second column would tend to be greater than those in the first, and the figures in the fourth column greater than those in the third.

Now the figures are such that, in comparing the two groups in a given method, if the second column be subtracted from the third (the second group excelling the other in the O. M. method), then the first must be subtracted from the fourth (the second group excelling the other also in the P. C. method). Where the case is reversed and the first group excels the second, then if the third column be subtracted from the second then the fourth must be subtracted from the first.

In the first case, if our main hypothesis is to be proved, viz. that column 2 > 1 and column 4 > 3, then (col. 3–col. 2) < (col. 4–col. 1).

This is not the case however either in judgments of weights or in judgment of handwriting.

Neither is it true in beliefs that (col. 2–col. 3) > (col. 4–col. 1) as would necessarily be the case if the main hypothesis were a valid one.

Thus if the figures used in this problem were sufficiently large to be significant this experiment would prove conclusively that in weights, handwriting and beliefs, the method which is done first does not tend in any way to improve the judgments made by the method which follows it a month later. The figures are, however, entirely too small to be of any practical significance.

The two methods correlate so clearly that there is no basis offered for comparing even the effect of one upon the other.

Summary

From the foregoing discussion of results we may conclude:

1. The variability of the specimens of a given series from their average positions is not influenced in any way by the fact that the series has been judged by one of the two methods rather than by the other. In this respect the two methods may be considered equally efficient.

2. The correlations between the average order of the given series and the objective order of that series are practically identical for the two methods. With respect to this aspect of the problem the two methods may be considered equally efficient.

3. The average correlation between the two methods for the three types of judgment is .987. This indicates the absence of any basis for preference of one method over the other—with respect to the general results obtained.

4. The individuals as a whole depart from their group average no more in one method than in the other. This proves the equality of the two methods as means of investigating average variability of the individuals of a group.

5. A single individual who is representative of his group in one method tends to be representative of his group in the other method also. This indicates the equal efficiency of the two methods as applied to an investigation of a problem of this sort.

6. The two methods disagree slightly in their indications as to the relation between ability to judge handwriting and ability to judge beliefs. An investigation might be made to test further the validity of this discrepancy.

7. The method which is done first does not tend in any way to improve the judgments made by the method which follows it a month later. This would indicate that neither of the two methods is necessary as a supplement to the other.

Conclusions

On the basis of these conclusions as to the efficiency of the two methods in their particular application we may say that one is in no way to be preferred to the other.

On the basis of historical and empirical criticism as to the relative merits of the two methods we may say

1. That the order of merit method is vastly to be preferred to the method of paired comparisons, from the standpoint of their relative demand upon the time and energy both of the experimenter and of the subjects.

2. That the order of merit method is to be preferred in

that it requires a subject to give each specimen of the series its own separate rank. (By the method of paired comparisons two, three or four specimens may be given the same number of choices and thus be indeterminate as to their real position in the series. The subject is not forced to make a choice between them.)

3. That Cohn's objection to the order of merit method that it fails to give a real quantitative ranking of the series, as does the P. C. method, has been successfully refuted by Thorndike who has proposed a method of determining the quantitative values of the specimens of a series on the basis of their curve of distribution.

4. That Titchener's theory that the method of Paired Comparisons is necessary for the internal control of introspection—and Des Bancel's observation that this method "est plus naturel et rappele les procedes ordinaires de choix auxquels nous recourons dans la vie et tous les jours" (15) take the method out of the realm of statistics and designate its own particular field of efficiency in a psychology of pure introspection rather than of statistical measurement.

If, as Bullough maintains (16), experimental æsthetics be simply a matter of introspection and absolutely divorced from statistics, then it would seem that this method could retain something of its former prestige as a means to the investigation of the introspective æsthetic reaction. Its efficiency, even in this line, however, is denied by Gordon, who, in a recent experimental work in æsthetics, dismisses the method peremptorily with the statement, "The method of paired comparisons was discarded after some trials. Any one who has tried it with æsthetic tests will recognize the serious objection against it that it so quickly exhausts the æsthetic reaction" (17). This statement as it stands has not been refuted. It remains for the introspector to decide whether the method of paired comparisons is to maintain its experimental existence even as a vehicle of æsthetic introspective analysis.

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THE SYSTEMATIC OBSERVATION OF THE PERSONALITY—IN ITS RELATION TO THE HYGIENE OF MIND

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CONTENTS.

Scope of the Problem

The object of this paper is to give an orderly presentation, susceptible to quantitative treatment, of the essential factors in the proper mental adjustment of the personality to its environment, specifying the character of healthy mental reactions as distinguished from unhealthy ones. But so long as the qualities involved here are not capable of direct experimental measurement, the only approach, of the quantitative character that science demands, is the evaluation of comparative judgments about them. Such a way of dealing with this class of data has had its greatest development in the method of measurement by relative position, one of the

[1] The divisions are after Hoch and Amsden's 'Guide to the Analysis of Personality' (unpublished).

major indebtednesses of psychology to James McKeen Cattell. The fundamental principle is that whatever are the topics for inquiry in the study of the personality, the information *must* be cast in such a form as to give a judgment of the quantitative relationship of the subject to other individuals.

Experimental psychology has carried the formal aspects of the problem to a high degree of perfection. Less help is derived on the side of content, of the special topics of inquiry, partly because the statistical complications of the problem absorbed the major share of attention, and also because the conventional subject-matter of psychological study is not of a character to readily lend itself to progress in that particular direction.

One has been dependent rather on the study of those individuals whose faulty mental reactions bring them into clinical contacts, where the personality is observed at closer range, and under fewer conditions of dissimulation. This view of the personality lays a somewhat different emphasis upon its factors than is found in the usual psychological analyses. Those based upon the ordinary experimental procedures are limited to elementary motor and intellectual measures which are notoriously difficult to interpret in a dynamic relation to the personality. But even with the more refined methods of standardized judgment, the tendency has been to consider the personality in a primarily social way; according as we who observe it react to it, whether one's characteristics are such as to make him a useful, efficient and successful member of the community. For the present problem it is more relevant to consider human qualities as they make for the individual's satisfaction with life, capacity to maintain a wholesome outlook on existence. One viewpoint deals with a man's value to society, the other with his personal adjustment to it. One's external success is particularly related to the intellectual and volitional spheres, the subjective balance being more especially an affair of disciplined affective life. The difference in standpoint is most concretely illustrated in the comparison of such schemata as the Cattell series with its admirably quantitative features, and the

original Hoch-Amsden "Guide," of previous reference, with its more specific and searching content.

The paper by Hoch and Amsden of subsequent reference, and the present paper, are intellectual descendants of this 'Guide.' The essential differences between the revised schema of Hoch and Amsden, and the present, are: (1) the Hoch-Amsden is not limited to quantitative treatment, therefore can, and does, question more specifically; (2) its classification has been largely rearranged in correspondence with the author's special system of psychology. Save for these considerations, the two schemata are in such close accord that in the parallel columns below, the topics of the Hoch-Amsden are included only where they especially *supplement* the quantitative series of the present writer. The topics of this latter may be considered, in nearly all cases, as also implicit in the Hoch-Amsden.

The term 'mental balance' is used in both static and dynamic senses. In the dynamic sense an individual is well-balanced when he is in good mental adjustment to his environment. Such mental balance is disturbed in various psychogenic difficulties, 'conflicts,' 'tangles;' being also a function of the environment in that individuals may be able to maintain their balance in simpler situations but not in more complicated ones. In this regard, traits are important not only for themselves, but according to their combinations. The same self-assertiveness, for example, that leads one individual to realize his aspirations, leads one less well endowed into situations he cannot cope with, thus strengthening the mental balance of the one, and undermining that of the other. The unhappy effects of an over-sensitive nature are somewhat offset by a forgiving disposition; incompetence by a lack of desire for better things. The dynamic side of mental balance is mental adaptation. In this sense, it is a problem for solution.

In the static sense, mental balance is a function, for measurement. We speak of an individual as the better balanced, the greater difficulties it takes to upset him, or the greater variety of situations to which he can adapt himself;

just as a well-'balanced' ship maintains its stability in the roughest water, while 'top-heavy' ones are endangered even in a moderate sea.

Our immediate *Aufgabe* is to bring together the various sources of material considered, and by their means to construct an outline of personality that shall concretely state the factors of importance to well-adjusted character, and make possible the direct comparison of personalities in quantitative terms. Such an outline deals partly with attributes of character that may be the cause of difficulties, as sensitiveness or self-consciousness, and partly with reactions that may be the result of difficulties, as bashfulness or evil-speaking. There is, of course, a good or vicious circle in these traits. These schemata are easier to follow if they are divided in some way, and a very suitable division seems to be that of the original Hoch-Amsden 'Guide,' which is followed practically verbatim save for one additional section of *Recreative Activities*. The divisions, of which there are fourteen, are not intended to be rigid, nor could they be made so. The topics considered under *Recreative Activities* are in many respects continuous with those of *Balancing Factors*. Certain points under *Attitude towards Others* are obviously related to *Adaptability*. No single characteristic can be absolutely separated from other characteristics, any more than a single act is the product of a single motive.

The manner of presentation is to enumerate under the title of each division first the topics that are arranged under it to form a part of the present system. They are formulated not in the simple names of qualities, as in the schemata of Cattell or Davenport, but in the form of questions, similar to the Heymans-Wiersma and the Hoch-Amsden; *e. g.*, *how well does he keep his word*, *how sociable is he*. This often gives a sharper definition than is possible through single names of qualities. Immediately following these are quoted under each section the topics that best fit under it from (1) the revised scheme of Hoch-Amsden (those of a *supplementary* nature only, *cf.* above), (2) the list of Heymans and Wiersma, (3) the series arranged by Professor Cattell, (4)

selections from the 'Trait-Book' of Davenport. These will further indicate the scope of each division, as well as different ways in which its topics may be approached. There is then added a brief discussion of each division, more closely defining the topics of the present schema, and the relation of each section to the others and the entire series.

As the successive topics for inquiry are enumerated, five personalities are described in terms of them, that the actual working of the schema may be illustrated. Naturally the information is in every case furnished by one intimately acquainted with the individual concerned. The method of notation is similar to that recommended for the Binet-Simon scale. The excess of the attribute, *in relation to the average*, is indicated by a + sign, its deficiency by a − sign. One or the other attaches to every topic enumerated. In the case of marked presence or predilection or correspondingly marked aversion the symbol (!) is appended to either sign, in case of doubt the symbol (?); *e. g.*, *how conscientious*, −?, how sociable, +!. This affords six steps, as follows:

+!, marked presence above ordinary,
+, distinct presence above ordinary,
+?, doubtful presence above ordinary,
−?, doubtful deficiency or aversion,
−, distinct deficiency or aversion,
−!, marked deficiency or aversion.

(If one wishes to indicate that the judgment rests upon information of doubtful sufficiency, this is conveniently done by a ? placed *before* the + or − sign. Absolute lack of data for judgment is represented by ×.)

These + and − signs are suggested for their naturalness, and the ease with which the associations are formed. It need scarcely be said, however, that any quantitative method of notation may be applied to such a series of inquiries. They may be graded on a percentage basis, compared with a standard objective scale, evaluated by whatever method the investigator may prefer or the refinement of his working conditions permit.

It would have been very agreeable if the topics could have been so worded that + signs should represent qualities advantageous to the personality, and − signs disadvantageous ones, but there are obvious linguistic as well as psychological difficulties. Finally, it will be remembered that the qualities are intended to be estimated, not directly from the informants' statements, *but from the examiner's judgments based upon more detailed questioning.* Both content and manner of informants' replies, as well as all incidental features, are proper aids towards arriving at a judgment, and a certain discretionary competence in dealing with the obtainable data must always be assumed.

In the present instances, it may be observed that two are comparatively good personalities, presenting only minor or well compensated difficulties; another less favorable, pre-

I. Intellectual Processes

	A	*B*	*C*	*D*	*E*
How easily does the person learn	+!	+?	+?	+?	+
How good a memory	+!	+!	+?	+	−?
What fund of information (relative to educational opportunity)	+	+?	−	+	+
How well able to observe	+!	+	−	+	+
How vivid mental imagery	+?	+?	+	−	+

Hoch-Amsden[1]	Heymans-Wiersma[2] (Abridged)	Cattell[3]	Trait-Book[4]
Is he considered to have good common sense?	Take things in readily	Intellect	Attention
		Clearness	Retentiveness (of memory)
Is his advice sought by others?	Broad		
	Special Talents	Originality	Selectiveness (of memory)
Does he plan with good foresight?	Good observer	Breadth	
	Ear for music		Sense-imagery
	Memory		Sound (tone-deafness?)
	Retain reading		
	Abstract speculation		
	Absent-minded		

[1] Hoch and Amsden, 'A Guide to the Descriptive Study of the Personality, with special Reference to the taking of Anamneses of Cases with Psychoses,' (N. Y.) *State Hospital Bulletin*, Nov., 1913, pp. 12.

[2] Heymans and Wiersma, 'Beiträge zur speziellen Psychologie auf Grund einer Massenuntersuchung,' *Zt. f. Psychol.*, 42, 1906, 81–127; 258–301, *et seq.*

[3] Cattell, 'Homo Scientificus Americanus,' *Science*, N. S., 17, 1903, 561–570.

[4] C. B. Davenport, 'The Trait Book,' Eugenics Record Office Bulletin No. 6, 1912. The number of schemata that could be treated in this way is of course indefinite.

senting more sources of difficulty, but still fairly well handled, and two decidedly unfavorable, with many pronouncedly harmful traits, not well reacted to.

The rôle of the intellectual faculties in the personality is a subordinate one, though in some ways they influence its development secondarily. As Birnbaum points out, there are numerous features of character, as the higher religious and æsthetic perceptions that are possible only in the presence of a superior intellect; furthermore the degree of intellectual capacity contributes somewhat to the position one reaches in life, and the complexity of the situations one is called upon to adjust. If one's intellectual powers lead him into situations beyond his capacity to deal with in other respects, difficulties of adaptation will follow just as when one's ambition leads him to strive to a higher level than his intellectual capacity will support. To one individual the intellect may provide an excellent balancing material, while in another it serves only to aid in the elaboration of difficulties and inadequate adjustments to them.

The facility of acquiring ideas and the ability to retain them being two fundamental traits of intellect, information regarding these may be sought through school records, the individual's readiness to 'take in things' in later life, as well as the general quality of memory. Some rating should be possible in respect to the general extent of present knowledge, always however in proportion to the opportunities which there have been to learn. To inquire into the breadth of intellectual pursuits, the presence of special talents, the power of attention or concentration, as well as the selective faculty of the memory, are all above suggested as helpful in elucidating these questions. The matter of originality in thought may also enter here, if one wishes to distinguish it from that of resourcefulness in action. In spite of the difficulties in scaling, the clearness of sense-imagery cannot be left out of account, for besides its immediate interest, it is very important in relation to *Einbildung*,[1] since the latter can be largely determined by the vividness of the imagery it

[1] *Cf.* Position towards Reality, p. 322.

involves. In fact, there are few more inclusive questions for the mental balance of an individual than the tendency of the reactions to be dominated by imaginal stimuli.

II. Output of Energy

	A	*B*	*C*	*D*	*E*
How much motor activity..................	+!	+	−	−!	+!
How talkative:..........................	−	+?	−	−?	−
How skilful with tools, needlework and the like:	+	−?	+?	+!	+?
What degree of bodily dexterity and grace.....	−	+?	+	−	−

H.-A.	H.-W.	C.	T.-B.
	Mobility Active or lazy Manual dexterity Conversation violent Conventional Raconteur Flighty Same stories often Extempore speaker Conversation moderate Slow speech Laugh often	Quickness Energy	Excitement *vs.* quiet Quickness *vs.* slowness Manual work Graceful or awkward Taciturnity *vs.* volubility Directness *vs.* discursiveness

As is seen, this section covers the expenditure of activity in its grosser forms only. Here are included the individual's general motor habits, that of restless expenditure of energy, over and above that needed for the matter in hand, liveliness as opposed to general motor repose and economy of effort, if not actual sluggishness. Special queries regarding motor gracefulness and manual dexterity are also included here, because it seems that purely motor accuracy and coördination are often especially inefficient in a class of neurotics. The degree of interest and proficiency in athletic sports is often of value in arriving at a judgment in these matters. The information should be based on a general view of the individual's quickness and efficiency in motor adjustments. Linguistic habits are also considered here,. and should be inquired into with some minuteness, because they are easy to judge, and seem closely related to deeper factors of make-up. The questions of Heymans and Wiersma review the matter very specifically and well. A person's manner of speech is a

not infrequent superficial means towards the estimate of character, and a full account of it is apt to be not far out of accord with the volitional side of the personality in general.

III. Self-assertion

	A	*B*	*C*	*D*	*E*
How much effort to shape surroundings	+!	+!	−	+	+
How independent of the opinion of others	+!	+!	−	−	+
How much tendency to assume leadership	+	+	−	+	+!
How ambitious in material things	+	+?	−	+	−
How able to bear up under difficulties and misfortunes	−	+	+?	+	+
How able to face crises	+	+!	−?	+?	+!
What inclination to face danger	+	+?	−	−!	+

H.-A.	H.-W.	C.	T.-B.
	Independent Ambitious Courageous Courage in sickness Easily discouraged	Independence Courage Leadership	Suggestibility Coolness in emergency *vs.* loss of head Dignity, presence, *vs.* lack of dignity Ambition *vs.* apathy Pluckiness *vs.* disheartedness Combativeness *vs.* submissiveness

Few factors of character are of such import to material advancement as those here included. While the amount of effort to shape surroundings can be very largely a function of more fundamental topics to follow (as under *Position towards Reality*) it is desirable to put this question separately owing to its general intelligibility and concreteness. The social rise of a self-effacing makeup usually takes place only on the ground of very exceptional intellectual or sentimental endowments. It should be possible to say how much the individual has been inclined to assume leadership among companions, both in the adolescent period and later, whether not up to, or beyond capabilities.[1] An allied factor is that of suggestibility in a broad sense, as given in the independence of thought from that of others, in contrast to dependence on outside opinion. The character of ambitions should be considered, whether of a material or mental character, because

[1] To what extent does one influence the doings of others, causing them to act in his own interests?

the former demand more self-assertion than the latter, which are usually seen in those of less active tendencies. It has seemed advisable to include here the questions of capacity in danger and misfortune summed up in the conception of *courage*. One should consider the general willingness (*resp.* tendency) to face danger, as from actively courting death to the careful avoidance of activities involving the risk of even slight physical detriment. Further aspects are the retained capacity for adequate reaction under adverse conditions,[1] the similar capacity in sudden emergencies or crises, as well as the manner of enduring physical pain.

IV. Adaptability

	A	*B*	*C*	*D*	*E*
How get along with other children	+?	+?	+	−	+
How get along with people in older years (tactfulness)	+	+	+	−	+
How conformable to discipline	+?	−?	+?	−!	+
What tendency to be guided by advice	+?	+?	+	−!	−?
How resourceful	+!	+	−?	−	+

H.-A.	H.-W.	C.	T.-B.
Is there a marked difference in his behavior in his intercourse with friends, family or strangers? When a child did he play freely with other children? Is he tactful or offensive? Is he quarrelsome, or easy to get along with? Can he coöperate with others? Does he readily adapt himself to new environments, as being away from home, moving to new places, etc.?	Change occupation	Coöperativeness	Resourcefulness *vs.* lack of resource Tactfulness *vs.* indiscretion Coöperativeness *vs.* aloofness Obedience *vs.* disobedience

Under the caption of *Adaptability*, Hoch and Amsden brought together certain broad themes of inquiry into the personality's adjustment to its environment. The standpoint is a synthetic one, taking the personality as a whole, and the general efficiency of its adaptive functions. No small importance attaches to the attitude which the per-

[1] *Cf.* James, 'Principles,' Vol. II., pp. 578–9.

sonality tends to inspire in others. Distinct unpopularity and inability to get along with playmates is an ear-mark of defective personality, more especially in childhood and adolescent days than later, because at the earlier time the harmful trends as well as the reactions of companions to them are more freely expressed. In later years, the difficulties are apt to find expression in ways that bring one less into direct conflict with the environment; Heymans and Wiersma suggest the question of stability in given lines of work, or tendency to changing occupation or situations. A similar test of character is the capacity to fit into organization, to learn to command by learning to obey, as opposed to a temperamental tendency to infractions of discipline. Hoch and Amsden also placed here the ability to take (*resp.* follow) advice; and while this topic also is perhaps covered under succeeding sections, it is retained as a function of adaptability, since this is also reflected in one's general amenability to the better judgment of others. The prompt ingenuity in meeting unaccustomed situations, the *Resourcefulness* of the 'Trait-Book' is a quality of some indirect value in social adaptation, though the immediate relation to the mental balance of the personality is not so distinct as in the other topics.

V. General Habits of Work

	A	*B*	*C*	*D*	*E*
How prompt in reaction to situations	+	+	–	–	+!
How systematic in work	+!	+	+	+?	+!
How executive	+	+!	–	–	+!
How persistent	+?	+?	+	+	+
How punctual	+?	+?	–	–	+

H.-A.	H.-W.	C.	T.-B.
Is he active or over-active by fits and starts? Is he committed to a routine, or is he free and agile mentally?	Put things off Impulsive Decisive Masterful Orderly Punctual	Will Perseverance Efficiency	System Alertness *vs.* Sluggishness Decision *vs.* vacillation Promptness *vs.* procrastination Perseverance *vs.* capriciousness

The precise character of work done for a livelihood is of course a part of all records. The extent to which the per-

sonality determines it is variable, because not everyone is free to enter the occupation he is suited for, and a suitable choice is not always made. Different motives may operate in choosing, the individual may be well adapted to his work in one way and badly in another; besides which, it is relatively incapable of modification. Far from being a reaction of adaptation, it may be an important source of difficulties. These sources of error may be counteracted by direct effects of occupation on character, and in the popular mind, certain temperaments are indeed firmly associated with certain types of occupation. We consider certain types of personality as best adapted to literary, artistic, teaching, scientific, professional, commercial, or laboring careers, as well as looking to find different sorts of mental balance in them; thus one speaks of the 'live wire,' the 'artistic temperament,' the 'fatal gift of music,' the 'brother to the ox.' The danger here lies rather in exaggerating the importance of occupation as a criterion of personality than of minimizing it. So far as the life-work can, in any given case, be regarded as an expression of personality, its factors are unusually complex. The most important would seem to be (*a*) the extent to which it is of material or mental rewards, (*b*) the moral conditions obtaining in it, (*c*) the extent to which it is a function of intellect, emotion or activity, (*d*) whether it is of a practical as opposed to an affective appeal, and (*e*) the severity and immediateness of the competition it involves.

Certain common factors of work are more susceptible to inquiry along the present lines. Stress is commonly laid on the *promptitude* with which situations are adjusted; whether the reactions are impulsive, rapid, deliberative, or slow. The question of orderliness and system in contradistinction to desultoriness may be considered here as a general attribute of behavior, also that of decision or vacillation. This latter applies rather to the single reaction to a situation, separate provision being made for the persistence in a given line of activity, the *perseverance vs. capriciousness* of the 'Trait-Book.' A useful distinction may also be drawn between the general rapidity of response as above, where none but the

individual is directly involved, and the punctuality in meeting obligations towards others. This attribute grades into the section following.

VI. Moral Sphere

	A	*B*	*C*	*D*	*E*
How well does the person keep a given word	+?	+?	+	+	+?
How truthful in matters relating to present or past	+	+	+?	+?	+
How trustworthy in money matters	+	+	+?	−?	+
How conscientious in the performance of duty	+	−?	+	+?	+!
How discreetly careful of the reputation of others	+	+	+!	−!	+
How mindful of the equal rights of others	+?	+	+!	+!	+

H.-A.	H.-W.	C.	T.-B.
	Neglect duties Loopholes in statements Frank with opinions Frank with intentions Trustworthy in statement Trustworthy in money methods	Integrity	Sincerity *vs.* insincerity Conscientiousness *vs.* unconsc. Honesty *vs.* dishonesty Truthfulness *vs.* untruthfulness

The consideration of this section presents some difficulties on account of the variable nature of the social criteria of morality. Furthermore, social morality does not necessarily run parallel to the mental hygiene of the individual. Many reactions that have a moral phase are included under other sections, there being placed under this one a small number concerned with some special standards of honorable conduct firmly grounded in the experience of the community.

In estimating qualities in this sphere it is regularly considered that the *moral* value of an act depends on the strength of the influences opposed to it in the person concerned. The test of truthfulness is the tendency to tell the truth to one's disadvantage, with the assurance of undetection in falsehood. Although society must be expected to react against transgressions of the social order as such, and recognize social conduct as such, without reference to its motivation, morality is from the present standpoint to be regarded rather as a subjective quality, to be measured in terms of resistance to opportunity. As in some individuals it is measured by a very moderate answer to an open, 'what is there in it for me,' so in others it has often proved beyond human power to measure.

As different forms of temptation vary in strength for different personalities, it is well to inquire into different phases of honorableness. The simplest one is of reliability in money matters, for here the issues are as a rule the most clearly defined. An important line of approach is also the extent to which agreements or promises are held to, even under difficulties; some keeping them to the last letter, others making them without due consideration if they will be able to keep them, some easily seeking reasons for not keeping them, or even making promises with the intention of evading them. Closely related to this is the broad question of conscientiousness in the performance of duty, the general sense of responsibility, within normal limits a necessary social quality, but in excess sometimes a burden to its possessor. It seems also that there is a morbid scrupulousness, or tendency to be blocked in action by considerations of a purely formal character, that is not of positive, but of negative moral implication, compensating for dereliction in large things by scrupulosity in more trivial ones.[1] And it is perhaps not out of place to assign a moral value to the quality of discretion, as 'the kind of honesty that keeps a man's mouth shut when he hadn't ought to be talking,' certain persons on the other hand being unable to keep confidences or yielding with marked readiness to the insidious satisfactions of ill-advised gossip, wanton traductions of character and the like. Dynamically, however, it seems rather different from the other topics. See table opposite page.

A considerable share of human activity is determined, or at least modified, not with direct reference to the struggle for existence, but with reference to certain immediate satisfactions involved. These are essentially the recreative activities, and derive their import, as reflections of the personality, through being chosen with relative freedom from a large number of alternatives, representing all varieties of activity. Their import is limited by the fact that in all but the leisure class, recreative activity may be largely a derivative of the work done for a living, selected precisely for its

[1] "Du sollst nicht auf meinen Mann schimpfen, Arthur! Ernährt er mich nicht?"

VII. Recreative Activities

To What Degree Are the Following Indulged In	*A*	*B*	*C*	*D*	*E*
Sports requiring quick and continuous action (tennis, motor-driving, sailing, etc.)	+!	+	+?	+	+!
Less active sports (golf, automobile riding, billiards, walking, etc.)	+	−	−?	−!	−
Hunting or fishing	−	−	−	−!	−
Camp-life in general	−	−	+	−!	+
Games of intellectual character (whist, chess)	−	+	+	−!	−
Games of less intellectual character (backgammon, hearts, casino)	−	−!	−	−!	−
Gambling or wagers	−	−!	−	−!	−!
Alcohol	−!	−?	−	−!	−!
Tobacco	−!	−!	+?	−!	−!
Other drugs	×	×	×	×	×
Reading	−	+	−	−?	−
Music	−	−	+?	−	+!
Pictures	+!	+?	+?	−	−
Artistic creations	×	×	×	×	
Delicacies in eating or drinking	+!	−!	−	+	−
Sports involving physical danger	−	−	−	−!	−

H.-A.	H.-W.	C.	T.-B.
In his play as a child, what did he prefer? Did he exercise much *imagination*[1] in it?	Fond of amusements outside home		Love of beauty
	Intellectual games		Knowledge *vs.* lack of intellectual interests
	Games of chance		Reading
	Sports		Mathematics
	Fads (sociological)		Music-rhythm
	Collections		Dancing
	Read much		Painting
	Fond of children		Exploration
	Fond of animals		Eating
	Drinking		Tobacco
	Eating		Alcohol
	Occupy leisure		Narcotics
	Change-loving or fixed habit		Money-getting
	Old memories		Money-hoarding
			Gambling
			Athletics
			Chess.

relation to the more fundamental activity. Thus a tendency to the more sensuous forms of recreation is frequently seen among persons of great practical energy and accomplishment. Again, the wild life may be sought for the pure sport of contest with nature, or it may represent a trend towards aloofness from social contacts that are distasteful. One does not, therefore, treat the avocations as fundamental, but as supplements or compensations to vocational activity, with account of the different mental mechanisms that may lie behind them.

[1] Italics mine.—F.L.W.

The enumeration of the varieties of recreative activity occupies a large share of the Heymans-Wiersma inquiry, the 'Trait-Book,' and some other schemata. Above it has been endeavored to modify these lists in accordance with what seem to be the essential differences between the types. We have to distinguish those recreations which depend primarily on motor adjustments, those depending primarily on intellectual adjustments, and those of a more directly sensuous character. Motor sports vary according as they demand rapid and more or less continuous adjustments, as tennis contrasted with billiards. The tendency towards the open air life should also be inquired into, and whether this is sought for its own sake, or for such secondary opportunities as hunting or fishing. The main line of cleavage in the intellectual pastimes is the complexity of the adjustments they involve, in which there is a fairly well-defined scale with chess and the whist group at one end, hearts, casino and the like, on the other.

In the sensuous group we usually consider music and art the more elevated, alcohol and drugs the more degraded.[1] In the course of the inquiry it will be well to note whether there is special tendency towards or avoidance of recreations that involve the element of competition. Here also belongs a question as to whether the pastime is followed for its own sake, whether it requires the additional stimulus of a wager, or whether the wager is itself the more fundamental thing. If a form of recreation is preferred that involves special danger, this too may throw a valuable sidelight on character.

Abstractly speaking, there would be no great disagreement about the relative value of these different recreations from the standpoint of mental hygiene. It is a banal observation that activity is better than passivity, and those which make the more real demands for adjustment to the outer world are accordingly to be preferred above those which do not; playing games that is, in contrast to watching them, reading, or listening to music. It appears that the mental value of these adjustments is indeed largely proportional to the motor

[1] Cf. also the sexual sphere, p. 324.

element they involve; billiards, bowling, tennis, for example, as opposed to card games. As pointed out, however, the recreative activities may be determined in too many ways to be an unequivocal index to the quality of mental balance. Their status is like that of the catalepsy which no one would overlook as a symptom, but no one would base a diagnosis upon.

VIII. General Cast of Mood

	A	*B*	*C*	*D*	*E*
How cheerful	−	+!	−	−	−
How stable	+	+?	+	+	−!
How deep	+?	−	−	−?	+

H.-A.	H.-W.	C.	T.-B.
Does he get despondent without apparent reason? Does he seem to enjoy his discomforts? When anxious what is his reaction? Has his mood apparently been permanently influenced by any special occurrence or circumstance? Did he have tantrums when a child?	Emotional (easily affectable) Happy or depressed Fearful Easily consoled	Emotion Intensity Cheerfulness	Ludicrousness *vs.* absence of sense of humor Anger *vs.* unruffledness Elation *vs.* depression Earnestness *vs.* frivolity Alternating mood *vs.* constancy

In dynamic considerations of the personality, the paramount rôle has been usually assigned to affectivity, especially by those who most closely observe the personality as such. We have to consider whether the affective life is manifested in ways beneficial or detrimental to the personality, and in what special processes it so manifests itself. Among the essentials of a perfectly balanced personality are that its acts shall accord with experience rather than with a contrary feeling, that is shall react to situations objectively rather than to their personal implications, that its aspirations shall be within and in accord with its capabilities, that its power of judgment shall not be impaired in crises of danger, or the incidence of good or evil fortune, and that there shall be no conflict between morals and conduct. The above traits have an important common factor in the affective sphere.

In the ill-balanced personality we find essential disharmonies between the affective reactions and those of the proper objective value. We find a behavior determined largely upon subjective grounds, by mechanisms akin to Bleuler's *autism;* a disturbance of the reaction's adequacy through being associated with abnormal affective states.

The examination of the individual's affective reactions begins with the inquiry into the fundamental affective basis of his reactions, the mood. The three directions which it takes are readily understood. First its reference to the pleasantness-unpleasantness scale, as represented by the *elation vs. depression* of the 'Trait-Book,' or the *Cheerfulness* of Cattell.[1]

The necessary relationship between this factor and the eternal circumstances is of course very slight. Either extreme is included in the pathological, as the constitutional excitement or depression. But the individual's mood is subject to fluctuations, so that its constancy or stability must also be ascertained; marked fluctuations may here also be quite independent of the external situation. Another feature of mood is that recognized by Cattell as *Intensity*, by Davenport as *earnestness vs. frivolity*. While this is an attribute usually viewed with respect from the social standpoint, it is often a prolific source of difficulties for the individual. "The world is a comedy to those who think, a tragedy to those who feel." And whoever feels the world as a tragedy, accomplishes the less to make it otherwise for his neighbor. Depth of feeling is apt to be associated with depression of feeling, just as in Kraepelin's phrase, the depressive emotions, as of fear, worry, despair, anger, and the like, provoke much intenser reactions than the greatest of joyful emotions, which rapidly subside into a placid sentiment of happiness assured.

Another brief but important section, within whose scope there is substantial agreement within the different schemata.

[1] This is one of the topics where it is especially important to remember that the average does not represent the midpoint of the scale or anything near it, because the regular and normal human mood is one of positive contentment with existence—not indifference towards it.

IX. Attitude towards Self

	A	*B*	*C*	*D*	*E*
How self-conscious	+	−	+!	+	−
How conceited	+	+?	−	+?	−
How patient, capacity to 'endure to the end'	−	−	+	+!	+
Demand for self-justification	×	×	×	×	+!

H.-A.	H.-W.	C.	T.-B.
Is he self-reliant or self-depreciatory (Feeling of inferiority)? How dependent is he for his comfort on the opinion others have of him Is he conceited—egotistic—given to self-admiration? Is he inclined to pay much attention to his aches and pains?	Obstinate in opinions Self-satisfied Talk of self Concerned for present or future	Unselfishness	Conceit *vs.* humility Self-confidence (in judgments) Patience *vs.* impatience

Foremost the question of self-consciousness, here asked of as a whole, but whose manifestations, as with other topics, are checked and rechecked through various succeeding rubrics. Easily comprehensible are the difficulties to which its excess exposes the personality, in the brooding over personal inclinations and wishes, over the consideration due from one's fellows, and the continual self-comparison with them. "There is too much ego in his cosmos." In view of the great importance of self-consciousness to personality, it may not be amiss to remark that it is among the very few of the present traits which seem now in fair degree open to experimental approach—through the methods of free association. The term *self-consciousness* is sometimes in a loose way used synonymously with diffidence, though a little reflection readily shows that, as in the character of Mr. Peter Magnus, self-consciousness is quite as truly an accompaniment of conceit. A self-conscious person may be either, largely according to the mood, extreme self-consciousness merely tending towards extremes in one or the other of these directions. One must therefore inquire separately regarding the tendency to self-esteem, self-confidence, or the 'feeling of inferiority.' And we here again observe what we observed

with the mood, that the normal, healthy estimate of self is not an indifferent one, but is distinctly on the side of a good opinion of self, together with cheerfulness of mood. Natural selection would probably tend to develop such attitudes, though the feelings of humility and unworthiness have proved by no means without their psycho-biological function. What was noted in an active sense as *persistence* is here included in the passive sense as *patience*, the ability (*resp.* tendency) to sacrifice the present for the future, to endure unto the end, to 'to wait till the sights come on, and then fire.' It is a simple and well-defined quality, expressly provided for in all the longer schemata.

X. Attitude towards Others

	A	*B*	*C*	*D*	*E*
How sympathetic	+	–?	+?	–	+
How generous	+	+	+?	–	+?
How critical	+	+	–	+	+?
How jealous	+	–	–	+	–!
How sensitive	+!	–	+!	+!	+!
How forgiving	+?	–	–	–	–?
How able to judge others	+!	+?	–?	–	–

H.-A.	H.-W.	C.	T.-B.
Does he keep friends long or does he give them up on slight provocation?	Sensitive	Kindliness	Contemptuousness *vs.* respectfulness
Is he sentimental in his friendships?	Fault finding		Sensitiveness
What qualities in others attract him?	Suspicious		Trustfulness *vs.* suspiciousness
Does he show any marked preference for or great dependence on any member of the family, or marked antagonism?	Tolerant (of opinions)		Gratefulness *vs.* ungratefulness
Has there been any change in this respect between childhood and adult life?	Easily reconcilable		Considerateness *vs.* inconsiderateness
	Sympathetic		Self-sacrifice *vs.* selfishness
	Judge of persons		

The import of most of these topics is self-evident. *Sympathy* ranges to the other extreme of pleasure in the misfortunes of others. The character of sympathy must also be considered in relation to its objects. It has been aptly

pointed out that an excess of sympathy for animal suffering is often associated with the absence of, is indeed a compensation for, similar feelings towards humanity. Heymans and Wiersma are careful to make this distinction in their questionnaire. As elsewhere, it must be borne in mind that sympathy, also generosity, are favorable qualities within certain limits only, and the extreme grades be allowed to indicate their presence "to a fault." Generosity must not be figured in material terms only, but as a general opposite of "closeness"; measured in the amount of sacrifice. The term *critical* is used in its restricted meaning, to denote the general readiness to take external situations at their face value, as in business relations. The question is whether the individual fundamentally tends to be suspicious, critical (demanding to be 'shown'), trusting, or naïve. Jealousy may be considered the opposite of tolerance, to be estimated upon the ordinary basis of jealousy of one's own opinions, friendships, or in various social relations. Its presence in extreme degree recognizedly predisposes to difficulties in social adaptation, in fact its extreme absence may sometimes be easier for the individual, though it too may prejudice his position in the external world. Sensitiveness, to an abnormal degree, is a quality that figures largely in delineations of psychopathic personalities. Its part in Ribot's classification of temperaments will be remembered, and it was most significant that 'active' should have been chosen to represent its opposite. It is measured by the intensity of affective reaction to the given situation, its extreme being seen in violent fluctuations from euphoria to anger or despondency upon trivial or even imaginary occasions, as well as exaggerated but often ephemeral and unproductive reactions of pity, enmity or the like, as contrasted with indifference or apathy. Objectively, its most unfavorable result in the individual is the seclusion in which he may seek to escape the emotional shocks to which ordinary life exposes him, the withdrawal into self from the world of concrete aspiration and accomplishment.

The quality of forgiveness, or the general reaction to injuries, is perhaps a little complicated to deal with in a single

topic. None better illustrates the distinction in the above mentioned 'social' and 'individual' viewpoints in the study of the personality. From a social standpoint one's essential concern is how much revengeful reaction we shall find in a person; does he attempt to give evil for evil, or not? We regularly rate the latter course as the higher one. Mental hygiene also, is concerned with the individual's capacity for 'getting square'—not with one's enemy, however, but with one's self. This may occur, according to the personality, either through requiting an injury (*abreagieren*!) or through forgiving it. While it is proper to ascertain which of these adjustments the individual is inclined to, and the latter may well be regarded as ethically higher than the former, neither are means which are mentally unfavorable to the personality, *so long as the feeling of injury is given up.* The attitude of forgiveness will not of course be confounded with the mere abstention from vengeance, which may be conditioned by incapacity or by fear. Here is seen the morbid reaction to injuries, in the form of nursed grudges, cherished, but never productive resentment. We thus have first the external question of whether one adjusts his injuries through revenge or forgiveness, and then the fundamental one of how far he is able to adjust them by either of these means at all. The latter is much the more important in all questions of mental balance. It might also be inquired whether the reaction to mental injuries (slights, insults) is especially marked in comparison to material ones, as indicating a temperament of exaggerated subjectivity. Material injuries are also more capable of objective adjustment (restitution).

Of special significance also is the ability to judge others (independently of social experience), taken direct from the Heymans-Wiersma series. It is one of those specific questions included because of secondary interpretations it makes possible. It is distinct from the above mentioned quality of criticism in that there is meant the general readiness to accept, while here is meant the capacity to understand the factors that make for acceptability. To understand the personality of others, one must freely recognize his own, and the indi-

vidual who continually suspects where he should trust, and trusts where he should suspect, does so because he sees others through the distorting glasses of unacknowledged self.

XI. Reactions to Attitude towards Self and Others

	A	*B*	*C*	*D*	*E*
How scrupulous of personal appearance	+	+	−?	+	+!
How sociable	+	+!	−!	−	+
How socially forward	−	+	−!	−!	−?
How demonstrative of emotion	+	−	+?	−!	−
What tendency to unburden	−	−	−!	−!	−
How great a demand for sympathy	−	−!	−	−	−
How much inclination to self-pity	−	−?	+	+!	−
How much pleasure in the success and enjoyment of others	+	+	−	−?	+
How much of a 'good loser'	+	+	+	−?	+
How much given to witticisms, epigrams, etc.	−	+?	+	−	−
What tendency to emphasize the good side of the environment	+	+	+?	−	+?
How even-natured (temper)	−?	+	+?	+!	+

H.-A.	H.-W.	C.	T.-B.
Does he have many friends or is he whimsical in making friends? If he prefers to be alone, how does he rationalize this? Are there special circumstances under which he goes away by himself, *e.g.* when reprimanded, criticized, or when something is required of him? Does he blame others for his faults? Is he apt to blame others for his own mistakes? If reticent, is he reticent generally, or in relation to certain topics? Is he more frank to certain people? How does he react to pleasure, good news, success? (Description of reaction.) How does he react to real trouble, such as bereavement, failure or success (*sic*), responsibility? (Description of reaction.) Does he make attempts to overcome his despondency or worrying? What was his reaction to the death of any member of the family? Is there any special tendency to cruelty, plaguing, tantalizing?	Vain Change friendships Witticisms Natural manner Associate with inferiors Different manner towards superiors and inferiors Flatterer Well informed about neighbors Miserly	—	Love of sympathy Vanity Benevolence *vs.* malevolence Enviousness *vs.* unenviousness Openness *vs.* secretiveness Forwardness *vs.* bashfulness Avarice *vs.* prodigality Wittiness *vs.* dullness Frankness *vs.* closeness Gregariousness *vs.* seclusiveness

Under this heading are brought together a number of topics that are largely functions of the two preceding sections, but which afford the opportunity to put the inquiries in a more specific and searching form. The range from foppishness or finickiness in appearance, habits or personal requirements to that of extreme slovenliness is first mentioned. Finickiness and the like in superficial things is sometimes of import as the compensation of laxness in deeper ones. The fondness for society, tendency to seek it or avoid it, is a feature whose essential character it is needless to dwell upon, and which should be most carefully dealt with. It may, of course, be motivated in different ways; Heymans and Wiersma particularize to know if the society of superiors or inferiors is preferred. Two closely related questions are first, that of the tendency to *talk* freely to intimates of one's difficulties (not necessarily to take advice), as opposed to bottling up and attempting to deal with the situation alone. Second, the tendency to freely exhibit one's *feelings*, or to keep them to one's self, to the extent perhaps of dissimulation, *belle indifference*. As also in the 'Trait-Book,' the distinction is made between the above simple fondness for society or aversion to it (*gregariousness vs. seclusiveness*), and the tendency to forwardness or bashfulness in social relations; this again is worthy of especially careful inquiry. We also distinguish from the simple tendency to talk of one's affairs, that of an active craving for sympathy and reassurance in them; some persons requiring to tell their troubles, but quickly resenting sympathy in them.

"Are you weak enough," Monte Cristo asked of Morrel, "to pride yourself upon your sufferings?" The tendency to self-pity is a characteristic reaction whose essentially morbid quality is well recognized in the popular mind, and a high value is placed upon exceptional freedom from it,

> Not with an outcry to Allah nor any complaining,
> He answered his name at the muster and stood to the chaining. . . .

Especially important questions are those dealing with the attitude towards the success and happiness, or the disappointments and failures of others. No one will get much

from life who dislikes to see others get more; and this reaction, often exquisitely shown in the intolerance of innocent amusements,[1] or a feeling of opposition on hearing others well spoken of, is not without its far-reaching consequences—or correlates—in the individual's outlook on existence. On the other hand, the capacity to provide for the enjoyment and well-being of others is generally regarded as a chief essential in the durable satisfactions of life.

One may get reliable information of a specific tendency to speak well or ill of one's surroundings. The former is an obvious evidence of better adaptation to the environment, if only on the metabolic level, that spares directly many minor difficulties. As a phase of this too may be noted whether the individual inclines to attribute good or bad motives to the actions of others, particularly if inclined to the latter. Still further in this field, the quality of a *good loser* is one that looms high in everyday estimation, with unquestionable reason. The conception is so definite in the mind of every one that it should easily prove one of the most useful topics in the series; as a popular measure of character, it probably outranks every other one. The quality is of course the greater, the greater reverses are met without mental flinching, and has its morbid converse in those who do not tolerate defeat in even minor sports, games, etc., in which they have no claim to superior ability, without the most ill-concealed evidences of discomfiture.

The tendency to minor clevernesses, repartee, jokes and the like, is one that receives attention in most detailed schemata. To treat this characteristic as an outcrop of fundamental cheerfulness of mood is an error against which it should not be necessary to caution at great length. Happiness expresses itself more spontaneously, and at lower levels. Wit and especially epigram are in the well-balanced personality the mere playthings of acute intellect. But to other and

[1] "We know the man," remarked the captors of Gabriel Grub, "with the sulky face and the grim scowl, that came down the street to-night, throwing his evil looks at the children, and grasping his burying spade the tighter. We know the man who struck the boy in the envious malice of his heart, because the boy could be merry, and he could not. . . ."

equally distinct personalities these things have a more essential relation, and serve the function of a 'cavalry screen' behind which deeper and less exhibitable trends of the personality are hidden. In those whom one knows well, one sees this mechanism quite too often to doubt its reality or interpretation. It is one of the less objective ways that the organism takes for the social adjustment of its difficulties.

XII. Position towards Reality

	A	*B*	*C*	*D*	*E*
What capacity to take things as they are	+?	+	+	−	−
What capacity to acknowledge mistakes or transgressions	+?	−	+	−!	+
How practical	+?	−?	−	+	+?
How influenced in action by likes and dislikes	+	+?	−?	+	−
What tendency to day-dreaming	−	−	−	−	+

H.-A.	H.-W.	C.	T.-B.
	Practical	Judgment Reasonableness	Phantastic Objectiveness *vs.* introspectiveness. Opinionatedness *vs.* diffidence

This has been made perhaps the most important and inclusive of the divisions. "We may," writes Professor Cattell, "imagine a world of Arabian Nights, or of Arthurian knights, or of metaphysical twilight, but those who should act as though they lived in such worlds would find themselves in those parts of the real world known as . . . ." Still, the author of this passage should be among the last to draw a sharp line here. All of us live to some extent, be it ever so little, in such worlds; and to the extent to which we place our activities with the real world will it "continue to honor the drafts that we draw upon it."

In the broad way in which it is conceived of here, position towards reality may be treated at many different levels. We may consider at the most superficial level, the way in which a number of individually trivial reactions are affected by the personal factor of like and dislike. The common factor in these topics is the ability to react in accordance with a 'pragmatic' reality, against inclination. When this latter is out of harmony with the wisest and most reasonable course,

the extent to which behavior is deflected by it is a measure of inadequate adjustment to reality. This phase of the function corresponds to what is ordinarily included under 'self-control.' It may be seen in the simplest passages of life. To read an interesting book in a bad light, to deliberately overeat of favorite dishes, to pursue favorite sport to exhaustion, or when convalescing from illness, before one's strength is suited to it, or any other voluntary sacrifice of physiological well-being to immediate pleasure, are among the ways in which faulty adjustment to reality manifests itself at this level. In general, how much does the individual's competence in a task depend upon his liking for it?

And how able is he to squarely face situations he does not like? The paradigma for this phase of the topic is familiar to all. The fox did not acknowledge the actual inaccessibility of the grapes, but salved his *amour propre* by persuading himself that they were sour; with the result that when he finally came to some that he might have reached, he had so firmly convinced himself of their generally inedible quality that he again let them go and missed his dinner altogether.

A similar process obtains in the intellectual sphere. To what extent are the individual's opinions really matters of (subjective) policy rather than of creed? To what extent does he see his mistakes and acknowledge them, or strike an attitude on a question and then hang to it, shutting the eyes to contrary indications? The 'mechanism of wishfulfilment' is a banality. Cæsar long since remarked the willingness of men to believe what they wish, making sound military use of it; and he is always a rare thinker who pursues his interests to their ultimate conclusion without some intellectual sacrifice upon the altar of the god of things as he thinks they ought to be.

The inability to acknowledge mistakes is of a piece with the inability to acknowledge transgressions of moral principle. The allegation of false motives for conduct is an elementary feature of hypocrisy, but one must not neglect its further rôle in mental adjustment by self-deception. This is the mechanism implicit in *qui s'excuse, s'accuse*. One individual may

do wrong directly, in response to overmastering organic or social impulse, going down before a temptation as Greek before Greek, in frank recognition of a situation beyond his strength. A less robust type of nature reacts by persuading itself that the involved action is right. It is this process which is seen in such rationalizations as the 'higher law,' the 'larger good,' 'affinity,' '*Sichausleben*' and the like. "All is fair in love and war," that is, everything is fair to get what you want badly enough. Such expressions are essentially reactions of self-justification, in natures able neither to avoid the sense of wrong-doing nor to endure it. How great is this demand for self-justification? Is it so great that the individual lies to himself in order to obtain it? It is impossible to count on 'unlimited elasticity,' as Adolf Meyer puts it, in the use of such mental adjustments as these. Occasionally, when a sudden stress of circumstance drives home an issue habitually evaded in these ways, flurries of an altogether psychotic nature result. Or there may be a gradual, but none the less morbid detachment from the external situation, and the 'substitution' of imagined ones.

Day-dreaming is not necessarily a pernicious mechanism. August Hoch points out that in this way, especially in early life, ideals and aspirations may take form that have an important and salutary influence in giving shape to the activity of later years. In various occupations, as the writing of fiction, one must put imagination to continued and systematic use, but it is not clear that this necessarily impairs capacity for the world of affairs. The process may however go much deeper, especially in the presence of a power of vivid imagery, as the Germans have it, *Einbildungskraft*, which gives to the imagined situations a degree of satisfying power that lends itself readily to gravest abuse. We thus direct special inquiry to the tendency to *unproductive* day-dreaming, systematized flights of fancy, in any direction. The detachment from the real world that arises from living in a world of imaginings, the continued recourse to this mental 'Easiest Way' to gratify ambition or longings, sooner or later brings about a considerable dependence on them for this purpose,

breaking up both desire and capacity for adequate reaction to external things.

The process is one well recognized in the psychopathic personalities. In an especially well-drawn picture of such a degenerative condition Kraepelin remarks:

"The imagery possesses great sensuous vividness, and forms ready associations. In consequence there easily develops an abnormal tendency to fantasies. Many love to paint for themselves imaginary situations and adventures in the smallest details, and take pleasure in assuming the rôles of princes and heroes at an age when this childish habit has regularly long since vanished."

And Birnbaum, again, voices the matter more abstractly:

"Since, indeed, in the normal (personality), the kind, content and direction of thought are essentially influenced by affective factors, so also in these natures are aroused especially those ideas which are in harmony with the needs of an over-developed emotional life; *i. e.*, unreal plays on thought and structures of fancy, to the exclusion of the unsatisfying ideas that accord with reality. To this is added an incoördinate distribution of the proper affective elements; intensively pleasurable feelings in flights and images of fancy, incomplete and pleasure-lacking feelings in the sphere of intellection and experience. All this is the expression in these cases of the predominance of imagination (*Einbildungskraft*) as opposed to logical and critical thinking."

The psychogenic interpretation of dementia præcox has proceeded essentially through this mechanism. "What," asks August Hoch, "is after all, the deterioration in dementia præcox if not the expression of the constitutional tendencies in their extreme form, a shutting out of the outside world, a deterioration of interest in the environment, a living in a world apart?"

The *position towards reality* then, depends upon the coordination of affect and proper reaction. It is adequate to the extent that proper external reaction is not sacrificed to internal personal feeling.

XIII. Sexual Sphere

	A	*B*	*C*	*D*	*E*
How forward towards the opposite sex		+?		–	–
How freely speak with intimates of own relation to question	–	–	–	–	–!
What is the prominence of the following sexual reactions:					
Normal intercourse	–	–?	–	–!	–!
Flirtation, love affairs, 'spooning,' etc.	–	–?	–?	–!	–!
Sexual trends in reading, art, conversation	–	–	–	–!	–
Masturbation and allied practises, sexual imagination. . .	–	–	–	–!	+?
Negativistic reaction (prudishness)	+	–?	–	+!	–?
What degree of contentment with existing sexual adjustments	+	+	+	+	–
How dominant in sexual relationships	+?	+	–?	–	–

H.-A.	H.-W.	C.	T.-B.
Is his personal attitude in harmony with his own sex (Tomboy, sissy, mother's boy, mannish, effeminate)? Is he attracted by older or younger persons of the opposite sex? When love affairs were broken off, what was the reason? What was the reaction to disappointments in love? Was he decided or wavering when the question of engagement or marriage came up? In marriage or other similar relationships, what is the attitude toward the partner? Is there, or is there not, a desire for children? Are there any perversions? Are there any idiosyncrasies towards food or odors?	Promiscuous or continent Obscene or sexual witticisms	—	Sex indulgence Philoprogenitiveness Constancy *vs.* fickleness Amorousness *vs.* frigidness Chasity (*sic*) *vs.* licentiousness Innocence *vs.* impurity Sex perversion

The same adaptive mechanisms that we have been discussing for the personality in general are met with on a more limited scale, but with much clearer definition, in the circumscribed sphere of sexual activity. In the amount and character of these reactions in the individual, the general feature of bashfulness or forwardness towards the opposite sex is first to be noted. But it is impossible to rightly judge the quality of sexual adaptation simply from the gross degree of sexual reaction. A habit of reaction may well meet the demands of one organism that would be quite inadequate to the balance of another. Some light must be thrown on the subjective contentment with the types of adjustment em-

ployed. Natures not content with their adjustments have thus far failed of adaptation, while those superficially content with inadequate adjustments give evidence of fundamental lack of adaptability in an objective sense. Some stress should be laid on the individual's capacity to talk sincerely of the subject with closest associates. This of course applies to the question only as it affects the individual himself, in no way to the mere habit of speaking along related lines. A further indication of subjective attitude is given in the expressed motivation of any absence of sexual reaction, whether on moral grounds, hygienic ones, or otherwise, and the consistency of this motivation with actual conduct.

Next is to be seen what use the individual makes of the more special types of adjustment. First, the extent to which the impulse is carred to normal conclusion, a matter that can be very concretely dealt with. One should know if sexual intercourse has been absent (— !), restricted to marriage (—), not so restricted (waywardness, 'wild oats'), (+) or especially promiscuous (+ !). What use is also made of other, equally natural, but less conclusive ways of reaction, ranging through the ordinary social contacts, flirtations, 'spooning,' love affairs and the like. This is a very wide field, including most of the normal adjustments that are open to many persons under existing social and biological conditions. It is a general experience that the best safeguard against difficulties of all sorts in this sphere is the association with healthy-minded individuals of the opposite sex. But there are further, less adequate adjustments in the form of sexual trends in language, art, and the like, also the whole group of autoerotic manifestations, ranging from the most casual bits of day-dreaming to habitual masturbation, psychic or otherwise. A widespread shrinking from sexual reactions seems to have a good deal the same psychobiological foundation as the wearing of clothes, and as little of a pathological character. But, like the autoerotic reactions, it takes on a morbid character when it becomes an end in itself, replacing the trends that lead to more social means of adjustment. In this form we know it as *prudishness*, which may be the expression of

great personal resistances or ill-faced lack of opportunity, and also a process to

> Compound for sins we're most inclin'd to
> By damning those we have no mind to,

which pays in autoerotic coin the blackmail that instinct exacts from weakness for the pretense of virtue.

The purpose of such inquiries is to give the best possible insight into the individual's sexual adjustments, and whether their hygiene is good or bad. In determining this it must be remembered that for the individual the standards of psychosexual hygiene in terms of conduct are not absolute. The wide individual differences in sexual constitution make it impossible to generalize. Different communities have different and sometimes very precise standards of conduct in this respect, but they are always social standards, and there are always some individuals who are incapable of adaptation to them. If such standards are genuine and rigidly adhered to such unadaptable individuals tend to disappear, but if the standards are grounded in hypocrisy and readily departed from, then they will multiply and create more or less serious social problems. Contemporarily, social evolution has outstripped biological, and the genuine requirements of psychosexual hygiene are in certain individuals not compatible with the genuine interests of their community. This is what makes the tremendous difficulty of guiding the *vita sexualis* of a neurotic personality between the Scylla of promiscuity and the Charybdis of abnormal fixations.

XIV. Balancing Factors

	A	*B*	*C*	*D*	*E*
How firm in religious beliefs	+	−	−	−	+!
How active in church work	−	−	−	−	−
How intense interests or fads other than already dealt with.	+!	+?	−	−	−
To what extent are ideals expressed	+?	+?	+?	−	+!
How much are they in harmony with actual trends	+	+	+	+	+
How adequate a balance is the final result of these means ..	+!	+	−?	−	−

H.-A.	H.-W.	C.	T.-B.
Is he superstitious?	Religious Active philanthropy Conduct consistent Great schemes	—	Religiousness *vs.* unreligiousness

The final division of *balancing factors* is based upon the interpretation of certain activities as compensations for hindrances that the personality encounters to behavior in ways most natural or agreeable to it. As we saw, our basal question is that of mental *balance;* what then, is the demand for special balancing material, how great and how salutary a part does it play in the makeup. Practically all activities in pursuit of immediate pleasure may have a balancing function, perhaps an extreme one, though the indulgence of these does not ordinarily transcend the demands of recreation. Such activity as of itself sufficiently absorbs the attention will always serve to divert the attention from, and to that degree adjust, or 'balance,' difficulties or troubles. The 'lowest-level' phase of the balancing mechanism is seen here in very special activities taken up in the conscious effort to 'get away' from definite, unpleasant mental situations. The relative effectiveness in this direction of different sports, amusements, etc., is an interesting study for itself, besides which

Es ist ein Brauch von Alters her,
Wer Sorgen hat, hat auch Likör,

and certain repeatedly ineffective sexual reactions often observed in psychopathic individuals appear also to belong here.

When, through circumstance and temperament, the personality is blocked in the pursuit of fundamental instincts or wishes, a most characteristic reaction is to cultivate some other, more realizable interest with the same ardor as attached to the former trend, and to endow it, so far as imagination will permit, with the same value for the personality that the former trend would have had, could it have found expression in the individual's life ('Substitutive Reactions'). The inordinate faddish pursuit of special interests, quite independent of, and out of proportion to their objective return, is sometimes only too obviously the reaction to more fundamental trends which can be only incompletely lived out. This seems particularly the case with interests in opposition to prevailing social tendencies, as for example, the propaganda for antivivisection.[1]

[1] Cf. Dana, 'The Zoophil-Psychosis,' *Medical Record*, March 6, 1909.

Mental adaptation through altered sense of values is seen in its fullest development on a far higher level, in the 'balancing factor' of human nature *par excellence* and for all time; that of faith exemplified in religion. While the earliest beliefs arose presumably as interpretations of natural phenomena, with only secondary moral application, it is difficult to avoid the conclusion that those religions have been the most successful which have performed the highest *balancing* function, in subordinating matters of cosmogony or meteorology to the more intimate needs of psychic life. Mental difficulties in all fields are lessened by a belief in ultimate happiness or 'justice,' in the hope perhaps of torment for one's enemies, by glorifying earthly suffering and abasement, minimizing material comfort and distinction. It is a general observation that religious faith is less important to those to whom life presents few difficulties; thus exclaims the psalmist, "*Because they have no changes, therefore they fear not God.*" The direct effects of religious ceremonial are not unimportant. The question is, however, not simply one of how great a part religion plays in the makeup, but rather of the extent to which religious faith and activity have in the individual a balancing function, are the response to failures of adjustment in other fields. Special educational conditions may produce much religious fervor that is not of a compensatory character, and has no relation to difficulties. A gradual growth of religious interests in relative maturity, or a religious conversion, are more significant in this direction, indicating a situation insufficiently compensated by the existing mechanisms of adjustment, for which a more adequate compensation is sought in the newer interest.

As we should expect, a mentally hygienic religion demands expression in conduct, faith justified by works. The pathological manifestations we see in excessive preoccupation with dogma that either inhibits activity, or distorts it into bizarre forms.

The essential thing to note is the adaptive nature of religious elements in our reactions. The consequent inquiries are first, how much of an attempt is made in this direction.

Second, what kind of an attempt is it, whether one of a broad-based faith and hope, or one of hair-splitting niceties of creed, ritual and symbolism; one productive of good or evil in the attitude towards others. And last, how successful is the attempt, how much of value these factors really contribute to the mental balance that the individual maintains.

The idealizing process is a more general phase of the same altered sense of values, and the objects that we endow with the highest value we term ideals. They assume a subjective value beyond that which they have for the individual's relation to the external world. An ideal may be regarded as primary or genuine when it is in harmony with a fundamental instinct-trend of the personality that holds it. Ideals of home, of work, of wealth and influence are usually of this nature. A secondary or false ideal is one which is opposed to the fundamental instinct-trends, and is derived through an effort to heighten the value of suppressing them, as determined through circumstance and temperament.

A primary, genuine ideal performs its balancing function in extreme valuation of a fundamental trend, such as the maternal instinct, to compensate for inferior social position, neglect by, or loss of, the husband; or again, the idealization of certain kinds of work to compensate for economic sacrifices they involve.[1] These are, as a rule, the more favorable,

[1] A most exquisite literary expression of this 'adaptive alteration of values' is found in Gogol's 'The Cloak' (tr. I. F. Hapgood), as follows:

It would be difficult to find another man who lived so entirely for his duties. It is saying but little to say that he served with zeal: no, he served with love. In that copying, he saw a varied and agreeable world. Enjoyment was written on his face: some letters were favorites with him; and when he encountered them, he became unlike himself; he smiled and winked, and assisted with his lips, so that it seemed as though each letter might be read in his face, as his pen traced it. . . . Having written to his heart's content, he lay down to sleep, smiling at the thought of the coming day,—of what God might send to copy on the morrow. Thus flowed on the peaceful life of the man, who, with a salary of four hundred rubles, understood how to be content with his fate; and thus it would have continued to flow on, perhaps, to extreme old age were there not various ills sown along the path of life for titular councillors as well as for private, actual, court, and every other species of councillor, even for those who never give any advice, or take any themselves.

. . . But he made up for it by treating himself in spirit, bearing ever in mind the thought of his future cloak. From that time forth, his existence seemed to become, in some way, fuller, as if he were married, as if some other man lived in him, as if he were

healthier adjustments in this sphere, because based on things that regularly do have positive and high value for the personality. The less favorable means of dealing with the difficulty are to dodge the existence of the obstructed trend by idealizing its opposite. This creates a secondary ideal, out of harmony with the real tendencies of the personality, to be, as one epigrammatist put it, a 'mirage of failure.' An ideal of temperance is false if it arises from an incapacity for indulgence; an ideal for poverty is false if grounded in the inability to accumulate wealth; an ideal of chastity is false if but the response to failure of sexual adaptation. The concrete results of such mental habits are only partially understood. General embitterment may ensue if the illusions are lost after having committed the individual to existence limited by them. Needless self-accusation may result from the personality's reassertions of itself in the face of standards it has falsely assumed. But it is also clear that the symptomatology of mental disease can be described, in no small part, in terms of such mechanisms as these.[1]

In the observation of the personality first account is taken of the general tendency of the individual to idealize. The normally satisfied personality probably has little tendency to idealize, and one does not meet with it save in the presence of corresponding difficulties in mental adjustment. As means of compensating for difficulties, it must be ascertained if the trends idealized are genuine, or out of harmony with the underlying nature of the individual. The former situation represents a healthy mode of adjustment, even though the difficulties be great. False ideals represent difficulties badly handled, and are more diagnostic of the presence of difficulties, since the self-deception they involve would scarcely be gratuitous. No phase of the inquiry demands more thorough

not alone, and some charming friend had consented to go along life's path with him,—and the friend was none other than that cloak, with thick wadding and a strong lining ncapable of wearing out. He became more lively, and his character even became firmer, like that of a man who has made up his mind, and set himself a goal. From his face and gait, doubt and indecision—in short, all hesitating and wavering traits—disappeared of themselves. Fire gleamed in his eyes: occasionally the boldest and most daring ideas flitted through his mind; why not, in fact, have marten fur on the collar? . . .

[1] The 'biogenetic psychoses.'

knowledge of the person, nor more penetrating judgment in weighting the obtainable information.

The concrete lesson for mental hygiene is to avoid all mechanisms of idealization that are not readily and directly convertible into action; in every case to be most careful that the things idealized are things really wished for, and not to form ideals in opposition to underlying tendencies of the personality, or allow them to be formed in others for whose mental welfare we are responsible.

Conclusion

Considering in respect to mental balance the personalities with which one's acquaintance is sufficient, there is observed in the first instance a reaction type not greatly burdened by internal difficulties, the makeup not harboring such traits as predispose towards them. Such personalities are comparable to ships well provided with proper charts and other equipment, which hold an accurate course through deep and open waters, disturbed only by such storms of fortune as may cross their path from without. Other personalities are hampered in their reactions by specifically unfavorable features of temperament, as hypersensibility, self-depreciation, ambition beyond capacity; but in whom clear vision and frank recognition of the difficulties provides a compensation adequate to preserving the individual's essential adaptations to life. Such is the case of a ship whose course is laid among many rocks and whirlpools, but which, provided with good compass, accurate charts and other instruments of navigation, may well come through safely, if painfully and with hardship. These types grade into still others in whom the unfavorable trends are reacted to in ways harmful to the personality, as when hypersensibility breeds ideas of unfairness, self-depreciation breeds lack of effort, and imagination (or drugs!) are called upon for what is unattainable in reality. This is to sail by false charts, with undependable steering gear, and with compass out of adjustment. What wonder if such a vessel grounds over the shoals of failure or blunders upon the reefs of psychosis?

The personality in its present conception denotes an *ensemble* of characters ordinarily regarded as much dominated by hereditary influence. There is no reason to discount the supposition that mentally healthy parents endow their children with a corresponding capacity and tendency for mental right-living; and that parents of psychopathic constitution may transmit to their offspring the same possibilities for mental upset as were manifest in themselves. Under such circumstances the individual starts out with a constitutional tendency to bad mental habits that would in any event vastly increase the educational problem. But in the first case the child grows up in a healthy domestic atmosphere, while in the second a bad heredity plays directly into the hands of a bad environment, providing a neurotic *milieu* in which undisciplined plays of imagination and emotion, with other harmful mental tendencies, find ready opportunity for their rankest growth. Under such circumstances the psychopathic personality probably owes its development quite as much to modifiable conditions of early childhood as to heredity or original nature. Extreme import therefore attaches to the minute study of all such early environmental factors (only child!) and tendencies as lead immediately towards unfavorable habits of mind.

We deal here with the description of finished products, but are at once led back to the inquiry of what were the specific conditions, hereditary, infantile, or educational, that caused such mental tendencies to develop.

What unfavorable environment causes, correct external influences can prevent. The individual must be decisively turned from the establishment of various often obscure means of subjective gratification, and educated to a rigid dependence upon action in the external world. Every effort must be made to ensure adequate sense for objective, social values in life, and aspirations in accordance with them; that mistakes, failures, and deficiencies are faced squarely, without self-deceptive subterfuge, and that moral energy is not dissipated in the maintenance of perverted standards of conduct. Thus shall we provide the less favored vessel with proper charts,

and a rightly adjusted compass. It may still remain a frequent experience that, through no fault of intellectual endowment, certain personalities are constitutionally unfitted to shape their own mental or social reactions, all attempts at independent activity involving them in direct conflict with external conditions, or in affective complications which they can never adjust in conformity with social order. "*Video meliora proboque, deteriora sequor.*" These vessels cannot keep the course under their own power, and require special surroundings whose situations are artificially simplified to meet their capacity. The management of psychogenesis for the most efficient balance of mental faculties and surrounding conditions is a question of transcending value for breadth of normal and pathological application, and in the vital character of the personal and social issues dependent upon it. The constructive problem of psychology is mental adaptation.

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ON THE ELIMINATION OF THE TWO EXTREME INTENSITIES OF THE COMPARISON STIMULI IN THE METHOD OF CONSTANT STIMULI

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The investigators who have been interested in the theory of the psychophysical methods, have concerned themselves with two general problems. In the first place, they have sought for a better theoretical understanding of the methods, and of the thresholds and other final values obtained by the use of them. Secondly, they have attempted to standardize each of these methods in such a form, that the time and labor required for obtaining and treating the data would be reduced to a minimum. This second problem has been of special importance for the method of constant stimuli, inasmuch as the time and labor required for experimentation by this method has been, until recently, disproportionately large. But the method of constant stimuli is exceedingly valuable, because we probably have a better theoretical understanding of it than of any other psychophysical method.

Urban attacked the problem of shortening the time required for the calculations involved in the practice of the method of constant stimuli, and in 1912 published certain tables, by the aid of which an investigator may perform the complete calculation of a threshold in less than twenty minutes, once he has obtained the data.[1] It is unlikely that one ever will be able to reduce the calculation to a briefer form than that made

[1] F. M. Urban, 'Hilfstabellen für die Konstanzmethode,' *Arch. fur d. ges. Psychol.*, XXIV., 1912, 236ff.

possible by the use of these tables. Hence it would seem that any further advance towards reducing the labor involved in the practice of this method must consist in altering the experimental technique for the collecting of the data. This may be done in either of two ways; (1) by satisfying oneself with a smaller number of judgments on each comparison pair; and (2) by reducing the number of pairs employed in the determination. In either case, it is of interest to know the effect of such an alteration upon the values of the thresholds.

The question of how many comparison pairs should be employed has not been settled satisfactorily. Of course it would be desirable to use as many as possible; but both the time and the energy of the investigator have their limitations. It seems that the question has been decided chiefly for reasons of convenience; most experimenters employed seven pairs.[1] The differences between the standard stimulus and the largest and the smallest comparison stimulus of the series are as a rule so large that the judgments 'heavier' and 'lighter' respectively have a very high relative frequency. It is a fact that such results have but little influence on the values of *h* and *c*, the two quantities which determine the psychometric functions. This suggested the idea of recalculating a series of data, after eliminating the extreme comparison stimuli, and of comparing the results with those obtained in the complete series. The quantities *h* and *c* are those employed in the calculation of the thresholds, with which all of the psychophysical methods are primarily concerned; and if the above-mentioned elimination produces little change in the relation of these quantities, the values of the threshold should likewise show but slight variation. That this is actually the case was shown in a recent paper.[2] It was found that the values of the thresholds show very little variation for the averages of a large group of experiments. While individual values for groups of 100 judgments

[1] L. J. Martin and G. E. Müller, 'Zur Analyseder Unterschiedsempfindlichkeit,' Leipzig, 1899, 6ff. F. M. Urban, 'The Application of Statistical Methods to the Problems of Psychophysics,' Philadelphia, 1908, 5ff. S. W. Fernberger, 'On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli,' Psychol. Rev. Monographs, XIV., No. 4, 6ff.

[2] S. W. Fernberger, 'A Simplification of the Practice of the Method of Constant Stimuli,' *Amer. Jour. of Psychol.*, XXV., 1914, 121–130.

on each comparison stimulus show at times rather large and apparently unsystematic variations, still for a large group, these variations tend to cancel one another. An examination of the variations in the measure of sensitivity,—the threshold of Volkmann,—for the extended and for the reduced series reveals a surprising uniformity. Indeed, these values are the more remarkable when we consider that the disregarding of the two extreme comparison stimuli has eliminated one third of the experimental data which forms the basis of the study. The conclusion is that, formally at least, the elimination of the two extreme comparison pairs from the classical form of the method of constant stimuli does not materially affect the values of the thresholds.

There may be a difference between data obtained in a short series and those obtained in a longer series from which the extreme differences were eliminated afterwards. In other words, it may be that the presence of very large differences influences the attitude of the subject. Hence, in order to determine whether the same relations held empirically, we devised the following experiment in the field of lifted weights.[1] In all we employed four subjects; those who kindly consented to act as subjects were Messrs. F. J. O'Brien, D. I. Pope, G. S. Snoddy and R. H. Wheeler, all graduate students in experimental psychology at Clark University. Of this group, two were already trained in our particular technique in lifted weights, as they had both acted as subjects throughout a long series of liftings during the preceding winter and spring. The other two subjects were entirely untrained in this form of experimentation. One of these latter showed great adaptability and his judgments were recorded after less than a ten minute period of practice on the first day. In the case of the other subject, nearly an hour's practice was given him before the hand movements became sufficiently automatic to warrant our recording the judgments.

The stimuli which we presented to the subjects were hollow

[1] The experimental work for this study was performed at Clark University during the autumn of 1913. My thanks are due to the subjects for their faithful and sympathetic coöperation, and also to Professor J. W. Baird, Professor F. M. Urban and Dr. S. C. Fisher for their many helpful suggestions.

brass cylinders, 2.5 inches in diameter and 1 inch high, which were closed at one end. In none of their dimensions did they show a variation greater than 0.001 inch. A different number had been stamped on each cylinder, with a small steel die, so that the experimenter could differentiate between the different stimuli. Thus our stimuli were of the same shape as those which have been employed in earlier investigations. They differ from previously employed stimuli, however, in the manner of weighting.[1] The latter have been loaded to the proper intensity with shot and paraffine, the paraffine being inserted in order to hold the shot stationary. But the paraffine is not entirely anhydroscopic and for this reason the stimuli vary in weight. Experimenters have usually corrected any stimulus which showed a variation of more than ten milligram from its proper intensity. Our cylinders, which were weighted with solder, were entirely anhydroscopic. They have been under observation and in use for about a year and none of them ever showed a greater variation than six milligrams.

We prepared a series of stimuli of the following intensities: (*a*) fourteen weights of 100 grams each, of which twelve served as the standard stimuli for each pair; (*b*) two weights each of the following intensities: 88, 92, 96, 104 grams; (*c*) single weights of 84 and 108 grams. We arranged this group of weights into two complete series; one, a classical extended series of seven comparison pairs in which the comparison stimuli weighed 84, 88, 92, 96, 100, 104 and 108 grams; and the other, a reduced series with 88, 92, 96, 100 and 104 gram comparison stimuli. The second series was exactly similar to the first except that in it the two extreme intensities of the comparison stimuli were eliminated. Each of these comparison stimuli was compared with a standard stimulus of 100 grams. We shall use the expression '*extended series*' to designate the complete series of seven pairs of comparison weights; and we shall refer to the second series of five pairs as the '*reduced series*.'

The manner of lifting was the same as that employed in our

[1] F. M. Urban, *ibid.*, 1ff. S. W. Fernberger, *ibid.*, 7ff.

former study.[1] The weights were arranged about the circumference of a circular table with a revolving top. By means of this arrangement each weight could be brought in successive fashion directly under the hand of the subject, and in this way the space error was eliminated. The time error was present in the first order; that is, the standard weight was always lifted first and the comparison weight second. This error was kept constant by controlling the motions of the hand by the beats of a metronome. The metronome was set at 92 beats per minute and the lifting of each stimulus covered a period of four beats.[2]

The disposal of our weights was as follows. Numbers from one to twenty-four were printed about the circumference of the table. The standard stimuli were placed on the odd, and the comparison stimuli on the even numbers of the table. Hence by a complete revolution of the table, we secured a judgment on each of the comparison pairs for both the reduced and extended series. The comparison stimuli were not arranged about the table in a haphazard manner; but their order was carefully planned so that (1) the subject would acquire as little knowledge as possible regarding the objective relation of the stimuli; and (2) so that the effects of the order of presentation would be minimized.

TABLE I

Table Numbers	First Arrangement	Second Arrangement
1 and 2	92	104
3 and 4	100	92
5 and 6	88	84
7 and 8	108	100
9 and 10	96	108
11 and 12	104	88
13 and 14	84	96
15 and 16	92	104
17 and 18	100	92
19 and 20	88	100
21 and 22	96	88
23 and 24	104	96

[1] S. W. Fernberger, 'On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli,' PSYCHOL. REV. Monographs, XIV., No. 4, 1913, 7–18.

[2] Cf. S. W. Fernberger, *ibid.*, 10f.

Two arrangements of the stimuli were used in the experiment, which are given in Table I. The first column contains the numbers on the stimulus table in pairs; the second and third columns contain the intensities of the comparison stimuli. The arrangement was changed after 200 judgments had been secured on each comparison pair. The subjects were not informed that the change was to be made; all subjects later reported not to have been aware of any alteration, which indicates that they had neither acquired any knowledge of the objective relations of the stimuli nor learned the order of their succession. Three complete series of judgments were usually secured from one subject, upon which he was allowed to rest for a brief period. This minimized the effect of fatigue. When experimentation was resumed, after the rest, the first two judgments were not recorded, this period being allowed for the hand movements of the subject to become more regular. The intensity of the weights which were lifted at the start of a series was varied. Furthermore, in order that the subject should have no knowledge of the objective relations of the stimuli which were being presented to him, the table was entirely screened from view; the hand of the subject passing through an aperture in this screen.

Immediately after the lifting of the comparison stimulus of each pair, the subjects gave a judgment in terms of the three categories lighter, equal and heavier. A lighter or heavier judgment indicated that the second weight of the pair was subjectively lighter or heavier than the first weight. The equality judgment included not only all cases of actual subjective equality between the stimuli, but also all cases in which the subject was unable to formulate a judgment of heavier or lighter,—the so-called doubtful cases.

With the arrangement of stimuli and the manner of procedure which we have described above,[1] we obtained from

[1] In the description of our experimental arrangement we have spoken as if 24 stimuli had been employed: 12 standard stimuli, 7 comparison stimuli for the extended series, and 5 for the reduced series. We did this for the sake of clearness of exposition, and of differentiating more sharply between the extended and the reduced series. In reality, however, we employed only 14 stimuli, a complete series of 7 pairs for the extended series of the classical form. For the extended series, we obtained a judgment

each subject 500 judgments on each of the comparison pairs for both the extended and the reduced series, making a total of 24,000 individual judgments. We believe that these data are sufficient in quantity to make our results conclusive. By this experimental arrangement, moreover, the experiments for the extended and the reduced series were mingled and performed at the same time, so that we may assume that they were made under similar conditions. The subjects were not informed as to the nature of the problem, or as to the intensities of the stimuli; and their reports indicated that they possessed no knowledge of the objective relations. Finally, by our choice of trained and untrained subjects, we believe that we shall be able to ascertain the relations of the thresholds both for the extended and the reduced series, for two distinct stages of progressive practice.

After completing our calculations we found that the results from one of our subjects were of such a nature that they must be treated separately; but nevertheless they were quite in accord with the results which we obtained from the other three subjects, so far as our main problem is concerned. We shall treat the results of this subject *in extenso* at the end of this paper.

Tables II., III. and IV. contain the observed relative frequencies of Subjects I., II. and III. respectively, both for the extended and the reduced series. These three tables show a similar arrangement. The 500 judgments obtained from each subject have been divided into five groups of 100 judgments each, in the order in which they were taken. The Roman

for each of the pairs; the top of the table was revolved so that the stimuli were presented to the subject in successive fashion. For the reduced series, the cylinders of the five central intensities of the comparison stimulus were presented in the same manner to the subject. When we came to one of the extreme intensities of the comparison stimulus, which we wished to eliminate, the table was swung through a larger arc so that the two weights—the standard and comparison stimuli of the eliminated pair—passed under the hand of the subject, and the standard stimulus of the next pair was actually presented to him. Seldom did the subjects report that they were aware of the table having been moved through a larger arc than usual. But the possession of this knowledge would not have aided our subjects, for the reason that they were ignorant of the problem and of the objective relation of the stimuli. By the use of 14 instead of 24 weights, our experimental technique was very much simplified, while apparently the validity of our method was not affected.

TABLE II

Groups	Extended Series																				
	84			88			92			96			100			104			108		
	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.
I.	0.97	0.02	0.01	0.94	0.02	0.04	0.73	0.09	0.18	0.65	0.12	0.23	0.22	0.06	0.72	0.18	0.06	0.76	0.06	0.03	0.91
II.	0.98	0.02	0.00	0.94	0.00	0.06	0.83	0.03	0.14	0.54	0.06	0.40	0.26	0.06	0.68	0.13	0.06	0.81	0.02	0.02	0.96
III.	0.93	0.06	0.01	0.96	0.03	0.01	0.87	0.06	0.07	0.41	0.31	0.28	0.30	0.17	0.53	0.10	0.09	0.81	0.03	0.07	0.90
IV.	0.97	0.03	0.00	0.94	0.05	0.01	0.81	0.14	0.05	0.36	0.33	0.31	0.21	0.23	0.56	0.11	0.16	0.73	0.08	0.08	0.84
V.	0.94	0.01	0.05	0.93	0.05	0.02	0.69	0.16	0.15	0.46	0.16	0.38	0.19	0.17	0.64	0.13	0.07	0.80	0.07	0.03	0.90

Groups	Reduced Series										
	88			92			96			1	
	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e
I.	0.94	0.03	0.03	0.58	0.15	0.26	0.53	0.08	0.39	0.36	0.
II.	0.94	0.05	0.01	0.69	0.08	0.23	0.47	0.08	0.45	0.32	0.
III.	0.97	0.03	0.00	0.84	0.12	0.04	0.38	0.27	0.35	0.25	0.
IV.	0.87	0.10	0.03	0.81	0.14	0.05	0.36	0.28	0.36	0.29	0.
V.	0.87	0.09	0.04	0.75	0.13	0.12	0.30	0.19	0.51	0.24	0.

TABLE III

Groups	Extended Series																				
	84			88			92			96			100			104			108		
	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.
I.	0.70	0.12	0.18	0.63	0.18	0.19	0.45	0.22	0.33	0.33	0.28	0.39	0.14	0.21	0.65	0.10	0.17	0.73	0.04	0.05	0.91
II.	0.71	0.14	0.15	0.70	0.15	0.15	0.51	0 28	0.21	0.36	0.29	0.35	0.10	0.40	0.50	0.05	0.29	0.66	0.02	0.11	0.87
III.	0.56	0.40	0.04	0.51	0.47	0.02	0.21	0.66	0.13	0.05	0.53	0.42	0.03	0.32	0.65	0.00	0.25	0.75	0.00	0.12	0.88
IV.	0.46	0.53	0.01	0.50	0.47	0.03	0.21	0.66	0.13	0.08	0.47	0.45	0.01	0.38	0.61	0.00	0.17	0.83	0.00	0.09	0.91
V.	0.52	0.44	0.04	0.46	0.45	0.09	0.15	0.65	0.20	0.04	0.36	0.60	0.01	0.15	0.84	0.00	0.13	0.87	0.00	0.03	0.97

Groups	Reduced Series										
	88			92			96			1	
	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e
I.	0.60	0.20	0.20	0.34	0.34	0.32	0.24	0.25	0.51	0.09	0.
II.	0.69	0.23	0.08	0.32	0.41	0.27	0.18	0.39	0.43	0.06	0.
III.	0.52	0.46	0.02	0.25	0.61	0.14	0.07	0.52	0.41	0.02	0.
IV.	0.53	0.45	0.02	0.19	0.65	0.16	0.03	0.57	0.40	0.04	0.
V.	0.52	0.43	0.05	0.15	0.58	0.27	0.05	0.40	0.55	0.02	0.

TABLE IV

Groups.	Extended Series																				
	84			88			92			96			100			104			108		
	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h	l.	e.	h.
I.	0.98	0.02	0.00	0.95	0.01	0.04	0.82	0.05	0.13	0.70	0.15	0.15	0.17	0.12	0.71	0.13	0.06	0.81	0.06	0.03	0.91
II.	0.99	0.00	0.01	0.97	0.02	0.01	0.88	0.03	0.09	0.72	0.04	0.24	0.23	0.12	0.65	0.22	0.06	0.72	0.03	0.00	0.97
III.	0.98	0.02	0.00	0.93	0.06	0.01	0.81	0.16	0.03	0.41	0.37	0.22	0.12	0.32	0.56	0.08	0.20	0.72	0.04	0.08	0.88
IV.	0.98	0.02	0.00	0.98	0.02	0.00	0.78	0.20	0.02	0.48	0.43	0.09	0.09	0.56	0.35	0.09	0.23	0.68	0.03	0.09	0.88
V.	1.00	0.00	0.00	1.00	0.00	0.00	0.89	0.08	0.03	0.67	0.23	0.10	0.08	0.51	0.41	0.04	0.21	0.75	0.00	0.02	0.98

Groups.	Reduced Series										
	88			92			96			1	
	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e
I.	0.91	0.02	0.07	0.55	0.15	0.30	0.37	0.21	0.42	0.11	0.
II.	0.90	0.04	0.06	0.55	0.14	0.31	0.34	0.11	0.55	0.30	0.
III.	0.92	0.06	0.02	0.80	0.13	0.07	0.43	0.29	0.28	0.16	0.
IV.	0.92	0.07	0.01	0.75	0.22	0.03	0.44	0.31	0.25	0.09	0.
V.	0.90	0.08	0.02	0.81	0.16	0.03	0.52	0.30	0.18	0.16	0.

numerals in the first columns indicate these groups. Three columns are given to each comparison weight, in which appear the frequencies of the lighter, equal and heavier judgments on that weight. Reading from left to right, the first seven groups of three columns give the observed relative frequencies for the different comparison stimuli of the extended series in the order of their intensity. The last five groups of three columns show a similar arrangement for the reduced series.

From these data, we have calculated three groups of thresholds for the directions both of increase and decrease. First, the thresholds for the extended series of seven pairs of comparison stimuli have been calculated. Second, formal values of the reduced series from the data of the extended series have been calculated, by disregarding the two extreme intensities of the comparison stimuli. Third, the thresholds from the observed relative frequencies of the reduced series of five pairs of stimuli have been calculated. We shall refer to these three types of values as the threshold values of the *extended series*, of the *reduced series calculated* and of the *reduced series observed* respectively. These thresholds were all calculated in the usual manner by an application of the method of least squares and the use of Urban's tables, noted above. By this method the investigator obtains two values—h and c—from the frequencies both of the heavier and lighter judgments for every series. These quantities determine the form and position of the curves of the psychometric functions for all three categories of judgment. Moreover the relation between them of c/h defines the value of the threshold. The threshold in the direction of increase is obtained from the frequencies of the heavier judgments. The threshold in the direction of decrease is obtained, when we employ the relative frequencies of the lighter judgments. In our present study, both thresholds were calculated for every group of 100 judgments in the extended and reduced series, both calculated and observed, for all four subjects. Thus it was necessary to calculate in all 120 thresholds. Several years ago the labor of such a task would have been entirely disproportionate to

the result obtained, but with the use of Urban's tables neither the labor nor the time involved were excessive.[1]

TABLE V

Groups	S_1			S_2		
	Extended Series	Reduced Series		Extended Series	Reduced Series	
		Calculated	Observed		Calculated	Observed
I.	97.07	97.03	96.00	98.78	98.59	98.06
II.	96.81	96.90	96.19	97.80	97.86	97.85
III.	96.22	96.65	96.49	99.65	99.43	99.85
IV.	96.33	95.80	95.70	100.00	99.73	100.27
V.	95.68	95.62	93.87	98.25	98.28	98.11

TABLE VI

Groups	S_1			S_2		
	Extended Series	Reduced Series		Extended Series	Reduced Series	
		Calculated	Observed		Calculated	Observed
I.	90.50	91.76	89.29	96.53	97.27	95.42
II.	91.16	92.20	90.20	98.99	99.91	98.58
III.	86.30	87.80	87.82	98.53	98.44	98.63
IV.	85.23	87.91	87.79	98.24	98.01	99.13
V.	85.50	87.28	87.41	95.62	95.66	96.21

TABLE VII

Groups	S_1			S_2		
	Extended Series	Reduced Series		Extended Series	Reduced Series	
		Calculated	Observed		Calculated	Observed
I.	94.66	94.83	94.12	98.55	98.30	96.50
II.	98.24	98.35	94.66	99.31	99.52	96.86
III.	95.64	95.46	95.52	100.46	100.18	99.86
IV.	96.01	95.97	96.46	102.01	101.85	101.77
V.	96.76	96.76	95.64	100.90	101.08	100.50

Tables V.–VII. contain the values of the thresholds for subjects I., II. and III. respectively. Each table gives these values for each of the three forms of calculation for every group

[1] For a detailed description of this form of calculation *cf.*: S. W. Fernberger, 'On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli,' PSYCHOL. REV. Monographs., XIV., No. 4, 1913, 29–38. F. M. Urban, 'Hilfstabellen für der Konstanzmethode,' *Arch. f. d. ges. Psychol.*, XXIV., 1912, 236ff. S. W. Fernberger, 'A Simplification of the Practice of the Method of Constant Stimuli,' *Amer. Jour. of Psychol.*, XXV., 1914, 124ff.

of 100 judgments. As before, the Roman numerals of the first columns refer to the subgroups of 100 judgments. In the next three columns are found the values assumed by the threshold in the direction of decrease (S_1); the first of these refers to the extended series, the second to the reduced series calculated, and the third column to the reduced series observed. The next three columns show a similar arrangement for the values assumed by the threshold in the direction of increase (S_2).

For any one subject, these values of the thresholds are very similar on the whole, but they are by no means identical. The individual values show certain unsystematic variations, such as are generally found in a series of this sort. No regular tendencies seem to be present for the thresholds in any one set, derived from any one of the three sources. A closer scrutiny of the three thresholds for the extended and the reduced series for any one of the subjects reveals an interesting similarity among the results. Whenever a subgroup shows a variation in either direction from the preceding subgroup for the extended series, a variation in the same direction occurs for the reduced series, both calculated and observed, in corresponding subgroups. This relation does not hold absolutely for all our subjects; still in the case of subject III. (Table VII.), it holds with but a single exception (Series V, S_1, reduced series observed).

The relations between the series become more apparent when we consider the quantities which are obtained directly from the values of the thresholds. The interval of uncertainty is defined as the distance between the two thresholds ($S_2 - S_1$). The threshold of Volkmann (which is recognized as the measure of sensitivity), is one half of this value [$(S_2 - S_1)/2$]. The point of subjective equality is defined as the average or mean of the two thresholds [$(S_2 + S_1)/2$]. These three values depend upon the thresholds and are easily obtained when once the thresholds are calculated. Now a psychophysical method is essentially a prescription for the collection of data and for their evaluation, so that the result enables one to compare the sensitivity of two subjects, or that of the same subject at dif-

ferent times or under different conditions. Hence the measure of sensitivity and the point of subjective equality are the final goals of these methods; the first gives the measure of sensitivity, while the second enables one to ascertain the extent of the effect of the constant errors. Hence these quantities may be used as a basis for discussing the variations under consideration.

TABLE VIII

Groups	Interval of Uncertainty			Measure of Sensitivity			Point of Subjective Equality		
	Extended Series	Reduced Series		Extended Series	Reduced Series		Extended Series	Reduced Series	
		Calculated	Observed		Calculated	Observed		Calculated	Observed
I.	1.71	1.56	2.06	0.85	0.78	1.03	97.92	97.81	97.03
II.	0.99	0.96	1.66	0.50	0.48	0.83	97.30	97.38	97.02
III.	3.43	2.78	3.36	1.72	1.39	1.68	97.94	98.04	98.17
IV.	3.67	3.93	4.57	1.84	1.96	2.28	98.16	97.76	97.99
V.	2.57	2.66	4.24	1.28	1.33	2.12	96.96	96.95	95.99
Average...	2.47	2.38	3.18	1.24	1.19	1.59	97.66	97.59	97.24

TABLE IX

Groups	Interval of Uncertainty			Measure of Sensitivity			Point of Subjective Equality		
	Extended Series	Reduced Series		Extended Series	Reduced Series		Extended Series	Reduced Series	
		Calculated	Observed		Calculated	Observed		Calculated	Observed
I.	6.03	5.51	6.13	3.02	2.76	3.06	93.52	94.52	92.36
II.	7.83	7.71	8.38	3.92	3.86	4.19	95.08	96.06	94.39
III.	12.23	10.64	10.81	6.12	5.32	5.40	92.42	93.12	93.22
IV.	13.01	10.10	11.34	6.50	5.05	5.67	91.74	92.96	93.46
V.	10.12	8.38	8.80	5.06	4.19	4.40	90.56	91.47	91.81
Average...	9.84	8.47	9.09	4.92	4.24	4.54	92.66	93.63	93.05

TABLE X

Groups	Interval of Uncertainty			Measure of Sensitivity			Point of Subjective Equality		
	Extended Series	Reduced Series		Extended Series	Reduced Series		Extended Series	Reduced Series	
		Calculated	Observed		Calculated	Observed		Calculated	Observed
I.	3.89	3.47	2.38	1.94	1.74	1.19	96.60	96.56	95.31
II.	1.07	1.17	2.20	0.54	0.58	1.10	98.78	98.94	95.76
III.	4.82	4.72	4.34	2.41	2.36	2.17	98.05	97.82	97.69
IV.	6.00	5.88	5.31	3.00	2.94	2.66	99.01	98.91	99.12
V.	4.14	4.32	4.86	2.07	2.16	2.43	98.83	98.92	98.07
Average...	3.98	3.91	3.82	1.99	1.96	1.91	98.25	98.23	97.19

Tables VIII.–X. show the values of these three quantities for subjects I., II. and III. respectively. As previously, the first columns indicate the subgroups of 100 judgments each. The next three columns give the values assumed by the interval of uncertainty for all three modes of procedure. In the first of these appear the values for the extended series. The next column contains these same values for the reduced series calculated. The third column contains the same values obtained for the reduced series observed. The next three columns contain a similar arrangement for the measure of sensitivity; and the last three, for the point of subjective equality. The bottom row of each table contains the averages of the numbers in the various columns.

Let us first compare the values obtained for the extended series with those obtained for the reduced series calculated. The results which are yielded by this comparison are very similar to the results of our former calculation.[1] If one then compare the measure of sensitivity for the different subgroups, one finds relatively great, and by no means regular, variations. The averages for subjects I. and III. are very similar indeed; the differences between the measures of sensitivity as obtained from the extended series, and those obtained from the reduced series calculated are respectively − 0.05 and − 0.03 grams. In the case of subject II., this difference is considerably greater, being − 0.68 grams. It will be noted, however, that the measure of sensitivity of this subject is relatively very large.

When the values assumed by the measure of sensitivity for the extended series and those for the reduced series observed are compared, one finds a similar state of affairs. The values for the subgroups show greater variations here than they did when the values of the extended series and of the reduced series calculated were compared. For subject I., a comparison of the averages for the extended and for the reduced series observed reveals the fact that the variation is larger; the difference being + 0.35 grams. For subject II., the difference between the values for the reduced series observed and that

[1] S. W. Fernberger, 'A Simplification of the Practice of the Method of Constant Stimuli,' *Amer. Jour. of Psychol.*, XXV., 1914, 125ff.

for the extended series is smaller than the calculated value in the same comparison; the difference being -0.38. We find for subject III. again a very close approximation; the value of the measure of sensitivity for the reduced series observed being only 0.08 grams less than that for the extended series. It might be of interest to note here that subjects I. and II. were our untrained observers, while Subject III. possessed a rather high degree of training in the technique of this experiment.

When the magnitudes of the point of subjective equality for the reduced series, both calculated and observed, are compared with those for the extended series, unsystematic variations such as we should expect are again found. A closer analysis shows that there existed in addition certain systematic variations; these will appear more clearly when we consider the averages for each subject. In the case of subjects I. and III., the points of subjective equality for the reduced series, both calculated and observed, are smaller than those for the extended series. In both cases the values of the reduced series observed show greater variation from those of the extended series than do the values of the reduced series calculated. In the case of subject II., the values of both of the reduced series are larger than the value for the extended series.

It is doubtful whether the method of constant stimuli may ever be employed properly for anything except an extended study in which more or less highly trained subjects are employed. The effect of progressive practice would seem to preclude the use of this method for short series; and this effect would also prevent the application of the method for anthropometric purposes. In an extended study, it is proper that a large number of judgments be obtained from several subjects. After the results for each subject have been treated separately, it is proper that the combined results for all subjects be considered; with the exception of those which must be eliminated because of abnormal variations. For example *cf.* the case of subject IV. whose results are to be treated separately. Table XI. contains the averages of these three final values for our subjects I., II. and III. The columns give, in order, the values

obtained for the extended series, for the reduced series calculated, and for the reduced series observed, in the order mentioned. The first line contains the values for the interval of uncertainty, and the second and third lines contain respectively the values of the measure of sensitivity and of the point of subjective equality.

TABLE XI

	Extended Series	Reduced Series	
		Calculated	Observed
Interval of uncertainty	5.43	4.92	5.36
Measure of sensitivity	2.72	2.46	2.68
Point of subjective equality	96.16	96.48	95.82

Our results reveal the fact that the value of the measure of sensitivity for the extended series is slightly larger than the values for the reduced series, calculated and observed. The difference between the value of the measure of sensitivity for the extended series and that for the reduced series calculated is − 0.26 grams; while the difference between the value of the measure of sensitivity for the extended series and that for the reduced series observed is only − 0.04 grams. The difference between the measure of sensitivity for the extended series and the reduced series observed may be disregarded as we might expect a difference of at least 0.26 grams, since such is the difference between the values of the extended series and the reduced series calculated. It is, of course, a mere matter of chance that the value of the reduced series observed should more nearly approximate the value for the extended series than does that for the reduced series calculated. The difference between the value of the extended series and that of the reduced series calculated is slightly larger than the corresponding difference obtained in our former study.[1] But in the present case, these variations are so small that they may be entirely disregarded in an extended study.

When we consider the averages of the points of subjective equality for our three subjects, we find that the value for the reduced series calculated is greater than that for the extended

[1] S. W. Fernberger, 'A Simplification of the Practice of the Method of Constant Stimuli,' *Amer. Jour. of Psychol.*, XXV., 1914, 128.

series, the difference being + 0.32 grams. This signifies that the higher values for subject II. more than cancelled the smaller values found for subjects I. and III. When we compare the value of the reduced series observed with that of the extended series, we find that the former is somewhat smaller; the difference being − 0.34 grams. Again we believe that variations so minute as these may be entirely disregarded.

From the above findings we conclude that an investigator using the method of constant stimuli is justified in using a less extended series of comparison stimuli than the classical series of seven pairs. For we have shown that the elimination of the two extreme intensities of the comparison stimuli does not produce any considerable effect upon the values either of the measure of sensitivity or of the point of subjective equality, when we employ a long series of experiments. Such variations as may occur seem to be of a chance character and they tend to cancel one another. Moreover, these variations are so small that they may be entirely disregarded. In our former study, we showed that the above conclusions hold from formal considerations alone; our present study indicates that the same conclusions hold upon the basis of an extended body of experimental data. The elimination of the two extreme intensities obviously reduces the time and labor required for the accumulation of the data by nearly one third. This reduction becomes all the more important when we consider that the method of constant stimuli can be employed only in an extended series of experiments.

We shall now turn to a consideration of the data which we obtained from subject IV. This subject possessed at the outset the same amount of training as subject III. A study of his reactions in the former experiment[1] reveals the fact that he did not show the variations from the normal which appeared in his reactions in the present study. Table XII. contains the observed relative ferquencies of the different judgments for this subject; Table XIII. contains his values of the thresholds;

[1] The results of this study have not yet been published. The experiments were performed during the winter and spring of 1913, and in them an attempt was made to secure an introspective analysis of the comparison consciousness involved in the judgment of small differences in lifted weights.

Table XII

Groups	Extended Series																				
	84			88			92			96			100			104			108		
	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.
I.	0.97	0.00	0.03	0.89	0.01	0.11	0.77	0.01	0.22	0.56	0.02	0.42	0.27	0.01	0.72	0.13	0.02	0.85	0.04	0.00	0.96
II.	0.98	0.00	0.02	0.92	0.00	0.08	0.72	0.00	0.28	0.68	0.00	0.32	0.38	0.00	0.62	0.23	0.00	0.77	0.09	0.00	0.91
III.	0.77	0.06	0.17	0.76	0.02	0.22	0.56	0.13	0.31	0.57	0.08	0.35	0.29	0.12	0.59	0.18	0.08	0.74	0.18	0.07	0.75
IV.	0.68	0.00	0.32	0.87	0.01	0.12	0.68	0.05	0.27	0.41	0.01	0.58	0.35	0.05	0.60	0.30	0.07	0.63	0.30	0.01	0.69
V.	0.76	0.06	0.18	0.84	0.04	0.12	0.64	0.05	0.31	0.42	0.07	0.51	0.38	0.01	0.61	0.20	0.02	0.78	0.23	0.06	0.71

Groups	Reduced Series														
	88			92			96			100			108		
	l.	e.	h.	l.	e.	h.	l.	e.	h.	l.	e.	h.	.	e.	h.
I.	0.86	0.00	0.14	0.58	0.01	0.41	0.39	0.00	0.61	0.34	0.00	0.66	0.22	0.00	0.7
II.	0.92	0.00	0.08	0.57	0.00	0.43	0.42	0.00	0.58	0.23	0.00	0.77	0.18	0.00	0.8
III.	0.78	0.04	0.18	0.65	0.07	0.28	0.42	0.12	0.46	0.30	0.05	0.65	0.25	0.02	0.7
IV.	0.78	0.02	0.20	0.73	0.00	0.27	0.33	0.08	0.59	0.33	0.02	0.65	0.29	0.02	0.6
V.	0.73	0.04	0.23	0.67	0.05	0.28	0.43	0.06	0.51	0.29	0.06	0.65	0.20	0.06	0.7

and Table XIV. contains the values assumed in his case by the interval of uncertainty, by the measure of sensitivity and by the point of subjective equality for all three of our classes of comparison. These tables are arranged in the same fashion as were the corresponding ones which we have discussed above. For the first two subgroups of 100 judgments each, our objective conditions were precisely the same as those for the other observers. In his first subgroup of 100 reactions, Subject IV,

Table XIII

Groups	S_1			S_2		
	Extended Series	Reduced Series		Extended Series	Reduced Series	
		Calculated	Observed		Calculated	Observed
I.	96.52	96.53	95.40	96.77	96.82	95.47
II.	98.12	98.14	95.11	98.12	98.14	95.11
III.	94.96	95.15	95.43	98.06	97.81	97.18
IV.	96.41	96.70	95.82	97.79	97.92	96.66
V.	95.80	95.91	94.93	97.53	96.92	96.55

Table XIV

Groups	Interval of Uncertainty			Measure of Sensitivity			Point of Subjective Equality		
	Extended Series	Reduced Series		Extended Series	Reduced Series		Extended Series	Reduced Series	
		Calculated	Observed		Calculated	Observed		Calculated	Observed
I.	0.30	0.24	0.07	0.15	0.12	0.04	96.67	96.65	95.44
II.	0.00	0.00	0.00	0.00	0.00	0.00	98.12	98.14	95.11
III.	3.10	2.66	1.75	1.55	1.33	0.88	96.56	96.48	96.30
IV.	1.38	1.22	0.84	0.69	0.61	0.42	97.10	97.31	96.24
V.	1.73	1.01	1.62	0.86	0.50	0.81	96.66	96.42	95.74
Average III., IV., VI.	2.07	1.63	1.40	1.03	0.81	0.70	96.77	96.74	96.09

gave an exceedingly small number of equality judgments. The interval of uncertainty is directly dependent upon the relative number of equality judgments, and in his first 100 judgments his values for the extended series, for the reduced series calculated and for the reduced series observed were respectively 0.30, 0.24 and 0.07 grams. In spite of these abnormal results we continued with the same objective experimental arrangement and we endeavored by means of non-suggestive questioning to ascertain the factor or factors which were responsible

for the abnormalities. The results of this questioning will be given later.

In the second subgroup of 100 experiments, we obtained the even more remarkable finding that not a single equality judgment was given during the whole 1,200 judgments which were obtained in our extended and reduced series. In this case the thresholds in the directions of increase and decrease coincide, and hence we obtain the result that the interval of uncertainty is zero, as is also the measure of sensitivity. This means that if we compare, with our standard stimulus, any varying comparison stimulus whatsoever, the latter would be judged heavier or lighter with a probability of 0.5 or over. And furthermore, this holds even though the variation be infinitely small. Such a state of affairs is obviously impossible from either theoretical or experimental considerations.

Accordingly after the first two subgroups of 200 judgments had been completed, we altered one factor in our objective experimental arrangement by reducing the interval between our different comparison stimuli from 4 to 2 grams. Hence for the last 300 judgments—the last three subgroups in the tables—Subject IV. was given an extended series of seven comparison stimuli whose intensities were 92, 94, 96, 98, 100, 102 and 104 grams; and he was given a reduced series of five stimuli whose intensities were 94, 96, 98, 100 and 102 grams. Our object in employing such a series of comparison weights was to obtain a greater frequency of equality judgments if this could be secured by means of the introduction of a greater number of stimuli within the limits of the normal interval of uncertainty. In this object we were successful in a certain measure, as appears from the size of the intervals of uncertainty for the last three subgroups of 100 judgments each. Nevertheless, when the reduced interval was employed, the values which were obtained for the measure of sensitivity were considerably smaller than the normal values obtained with the use of a four gram interval between the comparison stimuli. Furthermore, it will be observed that the relative frequencies for the different categories of judgments on the various comparison weights do not by any means conform to

the usual type of curves of the psychometric functions.[1] This is particularly true as regards the curve of the equality judgments. These then are our reasons for disregarding the data which we obtained from subject IV. in our consideration of the primary problem of this study, namely, the effect of the elimination of the two extreme intensities of the comparison stimuli from the classical series of the method of constant stimuli.

We believe that the explanation of the abnormal character of the results which we obtained from subject IV., is to be found in the subjective attitude of the observer. We instructed our observers that they should judge whether the second stimulus presented in each pair was subjectively lighter, equal or heavier than the first. On the basis of our questioning, however, we believe that this subject did not accept the above task but set for himself an *Aufgabe* of invariably detecting a difference between the stimuli which were presented to him. For this reason, he approached the problem with a different subjective attitude from that of the other subjects,—a difference in attitude which would account for his anomalous results.

Another feature becomes evident when we compare subject IV.'s interval of uncertainty for the first two subgroups with his interval of uncertainty for the last three subgroups. The fact that the employment of a two-gram interval between the comparison stimuli gave markedly greater values of the interval of uncertainty indicates clearly that the size of the interval between the comparison stimuli has a profound inverse influence upon the size of the interval of uncertainty. Our present data are not sufficiently extensive to justify us in making positive assertions regarding this inverse influence, but they do justify us in emphasizing the importance of this phenomenon as a problem for future research. If in the light of such research, this inverse influence proves to be a genuine

[1] F. M. Urban, 'The Application of Statistical Methods to the Problems of Psychophysics,' Philadelphia, 1908, 106ff. F. M. Urban, 'The Method of Constant Stimuli and its Generalizations,' Psychol. Rev., XVII., 1910, 229–259.

finding, it must clearly be regarded as indicating an inadequacy of the method of constant stimuli.[1]

The above results and discussion may be summarized as follows:

1. The subjective attitude with which the subject approaches the problem in lifted weights, or the *Aufgabe* which he sets himself, has a profound influence upon the size of his interval of uncertainty.

2. In an extended study by the method of constant stimuli, it is possible to eliminate the two extreme intensities of the comparison stimuli from the classical arrangement of seven pairs, without thereby affecting the values either of the measure of sensitivity or of the point of subjective equality to any marked extent. In a former study we showed that this statement holds upon the basis of purely formal considerations; and our present study verifies these earlier theoretical findings upon the basis of psychological experimentation. For the trained subjects the effect of the elimination of the two extreme intensities is slightly less than it is for the untrained subjects. But in either case the differences between the extended and the reduced series as regards the value of the measure of sensitivity and the point of subjective equality are so small that they may be entirely disregarded. Moreover, when we average the final values which were obtained for all of our subjects, we find that these differences tend to disappear almost entirely.

3. The elimination of the two extreme intensities of the comparison stimuli from the classical series employed with the method of constant stimuli obviously reduces the time and labor necessary for the accumulation of an adequate body of data by nearly one third.

[1] Since writing this paper, we find that the same fact has been noted by Warner Brown in the case of the ascertaining of the stimulus threshold for salt sensations. The Judgment of Very Weak Sensory Stimuli with Special Reference to the Absolute Threshold of Sensation for Common Salt. *University of California Publications in Psychology*. I., No. 3, 1914, 229–235.

A STUDY OF THE EFFECT OF BASKET BALL PRACTICE ON MOTOR REACTION, ATTENTION AND SUGGESTIBILITY[1]

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Statement of the Problem

Much discussion has been published within recent years as to the probable value of *athletics* for college students, especially certain forms of exercise as that of *foot ball* and *basket ball.* In this connection it occurred to the writer that by singling out certain physical and mental traits it would thus be possible to reduce the problem to a measurable basis. Accordingly the physical trait of *motor reaction* and the psychical traits of *attention* and *suggestibility* were selected as those which might reasonably be expected to show change through basket ball practice when persistently followed up for a season.

Questions Around which the Outcome of the Experiment Seemed to Center

Does persistent practice at basket ball affect the subject in respect to rate of *voluntary movement* and in respect to *involuntary control?*

Is persistent practice at basket ball conducive to more, or less, power to concentrate *attention?*

Is the effect of this kind of exercise, when persistently carried out, such as to render the subject more, or less, susceptible to influence by *suggestion?*

[1] The experimental work represented in this study was performed in the laboratory of the educational clinic at the University of Puget Sound during the winter of 1912–13. In this connection acknowledgment is due the five members of my class, who elected the course in clinical work, for assistance in tabulating the data and evaluating same: also, to Dr. Frederick E. Bolton, dean of the school of education, of the University of Washington, for valuable advice and suggestions while carrying on the experiment as well as for assistance in the preparation of the material for publication.

These were the specific questions which it was thought the experiment should answer directly. Of course the more general question as to the desirability of any particular form of the traits mentioned, as that of *fast* or *slow* motor movement, ability to *concentrate* attention, or *susceptibility* to influence through suggestion, while no doubt more important in a broader consideration of the problem, were nevertheless only incidental to this study.

Chart showing comparative changes in per cents during the period of study.

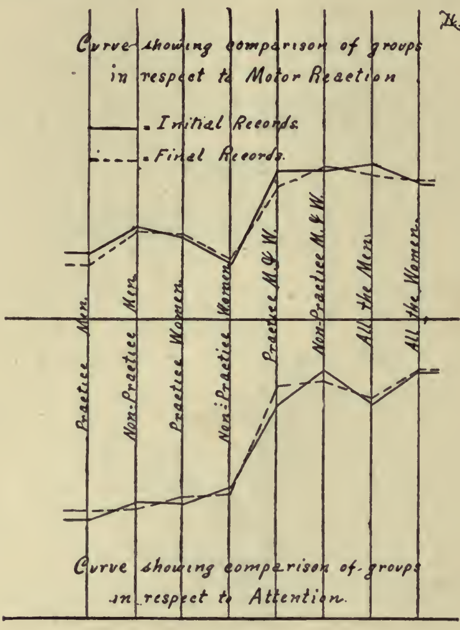
Subjects Coöperating

On the grounds that the element of coöperation would lend increased interest and hence add reliability to the data to be obtained, the proposed experiment was made known in a general way to the student body and volunteers were called for from which twelve, of as nearly equal ability as possible, were selected. The subjects were so chosen that they constituted four groups of three members each, arranged on the basis of sex, and also on the basis of their participating in basket ball, so that we had a practice and a non-practice group

of men and likewise a practice and a non-practice group of women.

Conditions of the Experiment

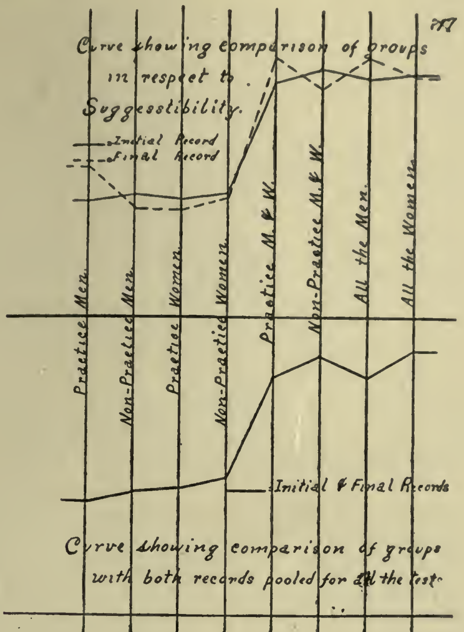
All the tests were conducted under conditions as nearly uniform as possible, those on *attention* being performed in class without disturbance or distraction, while those on *motor reaction* and *suggestibility* were done in the clinical laboratory.



A trained assistant was always in attendance in order to insure the utmost care in securing and recording the data.[1] The tests were all given at the very beginning of the basket ball season and then again three months later at the close of the study. Sufficient explanation and preliminary practice were allowed in order to work off curiosity so that each subject was in condition to do his best when the tests were given for the experiment. No visitors were allowed. In all the tests

[1] Acknowledgment is due in this respect to Miss Helen Lynwood Vent for her painstaking care throughout the experiment, who labored as an assistant in the laboratory without special remuneration for almost two years.

standard materials and apparatus were used, the same having been procured from C. H. Stoelting Co., of Chicago. Since all the tests used are found described in detail in Whipple's 'Manual of Mental and Physical Tests' it will be sufficient to



omit detailed description in this article and simply refer to them by number.

Key to the Following Tables

No.	Sex	Name	Age	Group	Trait	Name of Test Used	No. in Whipple's Manual
1	M	Bem	20	Practice	Motor Reaction	Rate of movement, Tapping	10
2	M	Wit	22	Practice		Steadiness, Involuntary Movement	12
3	M	Hor	20	Practice			
4	M	Hus	30	Non-Pr.	Attention and Perception	Cancellation, letter 'a'	26–*A*
5	M	Gen	35	Non-Pr.		Cancellation, letter 'q, r, s, t'	26–*B*
6	M	Buk	23	Non-Pr.		Cancellation, words with 'a, t'	26–*C*
7	W	Fog	26	Practice		Cancellation, Misspelled words	26–*D*–1
8	W	Bon	23	Practice		Cancellation, Misspelled words	26–*D*–2
9	W	Jon	24	Practice		Simultaneous Adding	29
10	W	Lan	23	Non-Pr.			
11	W	Sur	25	Non-Pr.	Suggestibility	Progressive Weights Illusion	41
12	W	Log	20	Non-Pr.		Progressive Lines Illusion	42

Table I

Test No. 10

Table Showing Record of all the Subjects in Respect to *Motor Movement*, as Measured by the Tapping Test

No.	(1) At the Beginning of the Study					(2) At the Close of the Study				
	A	*B*	*S*	*HI*	*E*	*A*	*B*	*S*	*HI*	*E*
1	7.2	6.5	6.9	.9	6.2	6.3	5.5	5.9	.9	5.2
2	7.7	6.3	7	.8	5.7	7.8	6.7	7.3	.9	6.2
3	6.6	5.8	6.1	.9	5.4	6.6	5.7	6.2	.9	5.3
4	6	7.9	6.9	1.3	9.1	7	6.6	6.8	.9	6.7
5	7	5.6	6.3	.8	5	8.7	6.7	7.7	.8	5.9
6	7.8	6.4	7.1	.8	5.8	7.3	6.6	6.9	.9	6.4
7	6.5	5.6	6	.9	5.2	6.9	6.5	6.7	.9	6.3
8	6.1	4.8	5.5	.8	4.2	6.4	5.3	5.8	.8	4.9
9	7.3	7.3	7.3	1	7.3	7.5	7.5	7.5	1	7.5
10	6.7	6	6.3	.9	5.7	6.9	6.5	6.7	.9	6.2
11	7.4	6.7	7.1	.9	6.4	6.8	6.2	6.9	.9	6.2
12	5.9	4.9	5.4	.8	4.4	5.9	4.9	5.4	.8	4.4

Rate of Motor Movement, as determined by tapping. Standard tapping board used, with kymograph for recording. Results based upon two trials each for the right and left hand over a period of twelve seconds. Own formula devised for evaluating data.

While the weakness of this formula is recognized, as is illustrated by reference to subject No. 4 in the first test, in which case the average number of taps for the left hand exceeds that for the right hand, yet it is believed that the correlation between the right and left hand should figure in the final index of efficiency in tests of this kind. Besides, this apparent weakness in the formula proposed may be obviated by reciprocating the term *B* over *A* in such cases as the one cited, or better still, by changing the formula to read *HI* equals the lesser term over the greater term. The following formula has been devised for this test:

A equals the average number of taps for the right hand. *B* equals the average number of taps for the left hand. *S* equals speed, when *S* equals *A* plus *B*, over 2. *HI* equals index of correlation, when *HI* equals *B* over *A*, or as suggested above, the lesser average over the greater average. *E* equals index of coefficiency, when *E* equals *HI* times *S*.

Table II

Test No. 12

Table Showing Record of All the Subjects in Respect to *Motor Control*, as Measured by the Steadiness Test

No.	(1) At the Beginning of the Study				(2) At the Close of the Study			
	A	*B*	*S*	*E*	*A*	*B*	*S*	*E*
1	62	69	65.5	34.5	11	28.5	19.8	80.2
2	35	45.6	40.3	59.7	111	126	118.5	—18.5
3	6.5	13.5	10	90	5	7	6	94
4	2	2	2	98	1	1	1	99
5	3.5	13.5	8.5	91.5	4	3	3.5	96.5
6	18.5	16.5	17.5	82.5	15	11.5	26.5	73.5
7	11	25	18	82	9.5	26.5	18	82
8	13.5	34.5	24	76	16.5	29	22.7	77.3
9	20.5	11	15.7	84.3	15	19.5	17.2	82.8
10	26.5	43	34.8	65.2	15	37	26	74
11	15	10.5	12.8	87.2	6.5	8.5	7.5	92.5
12	108	84	96	4	108	84	96	4

Steadiness of Motor Control, as determined by involuntary movement. Standard steadiness tester used, with kymograph for recording. Results based upon two trials each for the right and left hand, through holes No. four to No. seven inclusive, over a period of twelve seconds. Own formula devised for evaluating data, as follows:

A equals average number of errors for the right hand. *B* equals average number of errors for the left hand. *S* equals steadiness, when *S* equals *A* plus *B*, over 2. *E* equals index of coefficiency, when *E* equals 100 − *S*.

In this formula the correlation between the right and left hand is not taken into consideration as was the case in test number 10, still a careful comparative study of the two tables does not argue in favor of the omission.

Table III

Test No. 26 *A*

Table Showing Record of All the Subjects in Respect to *Degree of Attention*, as Measured by Cancellation of the Letter 'a'

No.	(1) At the Beginning of the Study						(2) At the Close of the Study					
	O	*C*	*W*	*S*	*A*	*E*	*O*	*C*	*W*	*S*	*A*	*E*
1	0	35	0	911	1	911	0	44	0	1,086	1.	1,086
2	0	27	0	776	1	776	3	35	0	1,100	.92	1,012
3	1	32	0	877	.97	850	2	34	0	985	.94	930
4	1	43	0	1,215	.98	1,191	4	46	0	1,325	.92	1,229
5	1	37	0	1,080	.97	1,048	1	39	0	1,100	.98	1,073
6	9	37	0	1,215	.8	972	4	39	0	1,170	.91	1,072
7	4	30	0	900	.88	794	2	33	0	945	.94	888
8	0	38	0	1,057	1	1,057	0	50	0	1,325	1	1,325
9	10	48	0	1,282	.8	1,063	0	46	0	1,215	1	1,215
10	0	44	0	1,086	1	1,086	3	46	0	1,310	.94	1,329
11	2	44	0	1,215	.96	1,166	2	44	0	1,225	.96	1,172
12	0	50	0	1,327	1	1,327	0	50	0	1,327	1	1,327

Degree of Attention, as determined by cancellation of the letter 'a' from a printed text containing type set in chance order. Standard printed forms used.

Formula taken from Whipple's Manual, as follows: *O* equals the number of letters omitted. *C* equals the number of letters cancelled. *W* equals the number of letters wrongly cancelled. *S* equals the number of letters covered. *A* equals the index of coefficiency, when *A* equals *C* minus *W*, over *C* plus *O*. *E* equals efficiency, when *E* equals *S* times *A*, or when *E* equals *S* over *A*.

Table IV

Test No. 26 *B*

Table Showing Record of All the Subjects in Respect to *Degree of Attention*, as Measured by Cancellation of the Letters 'q, r, s, t'

No.	(1) At the Beginning of the Study						(2) At the Close of the Study					
	O	*C*	*W*	*S*	*A*	*E*	*O*	*C*	*W*	*S*	*A*	*E*
1	12	95	0	679	.89	603	15	128	0	870	.90	783
2	11	80	1	594	.87	516	27	75	0	680	.74	500
3	4	81	0	559	.96	531	5	82	0	573	.94	539
4	16	144	0	1,016	.90	914	18	125	0	870	.87	757
5	28	106	0	830	.79	657	10	101	0	700	.83	581
6	29	95	0	79	.77	604	35	68	0	650	.66	394
7	52	94	0	931	.64	588	7	96	0	670	.93	623
8	30	95	0	800	.76	608	15	100	1	700	.86	602
9	40	99	0	832	.71	591	27	101	1	730	.82	527
10	30	38	0	1,066	.86	919	21	102	0	770	.83	639
11	18	124	0	893	.87	780	11	107	0	731	.88	645
12	22	111	0	838	.83	693	22	111	0	838	.83	693

Degree of Attention, as determined by cancellation of the letters 'q, r, s, t' from a printed text containing type set in chance order. Standard printed forms used. Formula taken from Whipple's 'Manual,' being the same as that used in test 26 *A*.

Table V

Test No. 26 *C*

Table Showing Record of All the Subjects in Respect to *Degree of Attention*, as Measured by Cancellation of Words Containing 'a and t'

No.	(1) At the Beginning of the Study						(2) At the Close of the Study					
	O	*C*	*W*	*S*	*A*	*E*	*O*	*C*	*W*	*S*	*A*	*E*
1	1	29	0	190	.96	184	2	32	1	190	.95	181
2	3	17	0	152	.85	129	7	22	1	198	.72	143
3	1	17	0	135	.94	126	2	22	0	164	.92	150
4	1	33	0	250	.97	243	2	32	1	190	.95	181
5	0	21	0	155	1	155	3	30	0	249	.90	224
6	1	26	0	190	.96	182	2	26	0	178	.93	165
7	6	18	0	164	.75	123	3	21	0	207	.88	181
8	0	28	0	200	1	200	2	35	0	259	.95	246
9	5	32	0	258	.87	222	5	31	0	224	.86	193
10	0	36	0	261	1	261	4	41	0	291	.91	271
11	4	28	0	260	.87	226	3	32	0	241	.91	219
12	0	30	0	198	1	198	0	30	0	198	1.00	198

Degree of Attention, as determined by cancellation of words containing the letters 'a and t' from a Spanish text. Standard printed forms used. Formula taken from Whipple's 'Manual,' being the same as that used in test 26 *A*.

Table VI

Test No. 26 *D*–1

Table Showing Record of All the Subjects in Respect to *Degree of Attention*, as Measured by Cancellation of Mis-spelled Words

No.	(1) At the Beginning of the Study						(2) At the Close of the Study					
	O	*C*	*W*	*S*	*A*	*E*	*O*	*C*	*W*	*S*	*A*	*E*
1	9	89	1	397	.90	357	8	75	1	335	.89	298
2	32	48	1	285	.59	168	14	51	1	250	.77	193
3	8	75	1	335	.89	298	12	55	0	250	.82	205
4	11	89	2	405	.73	296	5	62	0	272	.93	252
5	17	29	0	109	.63	69	12	27	0	170	.69	117
6	40	60	1	405	.59	240	40	43	1	318	.51	191
7	9	91	1	405	.90	365	4	72	1	294	.93	279
8	18	82	0	405	.82	332	5	65	0	282	.93	268
9	10	90	1	405	.89	361	12	91	3	392	.85	340
10	20	80	1	405	.79	320	17	64	1	315	.79	249
11	28	72	1	405	.73	296	8	58	1	257	.86	128
12	8	73	0	349	.90	314	8	73	0	349	.90	314

Degree of Attention, as determined by cancellation of mis-spelled words from English text. Standard printed forms used. Formula taken from Whipple's 'Manual,' being the same as that used in test 26 *A*.

Table VII

Test No. 26 *D*–2

Table Showing Record of All the Subjects in Respect to *Degree of Attention*, as measured by Cancellation of Mis-spelled Words

No.	(1) At the Beginning of the Study						(2) At the Close of the Study					
	O	*C*	*W*	*S*	*A*	*E*	*O*	*C*	*W*	*S*	*A*	*E*
1	6	64	0	290	.91	206	5	54	0	234	.91	213
2	3	43	1	183	.91	167	22	43	2	265	.65	172
3	5	54	0	234	.91	213	13	75	0	350	.85	298
4	2	72	0	305	.97	296	2	77	1	321	.96	309
5	13	21	0	127	.62	79	19	27	0	171	.59	100
6	20	54	0	305	.73	223	35	51	0	336	.59	199
7	2	72	1	305	.96	293	0	78	1	309	.99	306
8	8	61	3	277	.84	233	10	70	3	318	.84	266
9	10	90	2	396	.88	246	9	93	2	400	.90	360
10	10	51	2	252	.80	202	10	68	1	320	.89	284
11	5	54	0	234	.91	213	5	58	0	250	.92	230
12	11	48	0	234	.81	190	11	48	0	234	.81	190

Degree of Attention, as determined by cancellation of mis-spelled words in English text. Standard printed forms used. Formula taken from Whipple's 'Manual,' being the same as that used in test 26 *A*.

Table VIII

Test No. 29

Table Showing Record of All the Subjects in Respect to *Degree of Attention*, as Measured by the Simultaneous Adding Test

No.	(1) At the Beginning of the Study								(2) At the Close of the Study							
	A	*B*	*A'*	*C*	*S*	*W*	*E*	*C'*	*A*	*B*	*A'*	*C*	*S*	*W*	*E*	*C'*
1	33	18	15	24	90	0	90	1.2	38	18	17	29	102	0	102	1.2
2	32	21	18	24	95	1	85	1.1	36	15	12	26	89	0	89	1.2
3	37	19	15	26	97	0	97	1.2	48	12	15	23	98	0	98	1.8
4	54	39	18	32	143	3	113	1	51	18	18	46	133	3	103	1.1
5	25	22	6	14	67	4	27	.9	35	11	12	21	79	5	29	1.5
6	21	12	10	18	61	1	51	1	24	9	12	12	57	0	57	1.7
7	29	18	11	18	76	1	66	1.1	45	18	6	24	93	1	83	1.2
8	30	18	15	23	86	1	76	1.1	30	9	9	20	68	0	68	1.3
9	36	30	11	15	92	2	72	1.1	45	12	19	24	100	7	30	1.9
10	39	18	15	22	94	2	74	1.3	39	18	18	29	94	1	84	1.2
11	26	17	6	13	62	0	62	1.1	24	16	9	16	65	2	45	1
12	33	22	6	26	87	0	87	.8	33	22	6	26	87	0	87	.8

Degree of Attention, as determined by simultaneous adding of three columns of figures. Standard printed forms used. Own formula devised, and adaptation of method. Results based upon adding 'Signal A' for one minute, 'Signal B' for one half minute, 'Signal A' again for one half minute, and then 'Signal C' for one minute.

The following formula has been devised for this test, viz: Signal A equals 1, 2, 3. Signal B equals 2, 3, 1. Signal C equals 3, 1, 2. A equals the number of additions of signal A. B equals the number of additions of signal B. A' equals the number of additions of signal A the second time. C equals the number of additions of signal C. S equals the total number of additions. W equals the number of errors. E equals the index of efficiency, when E equals S minus 10 times W. C' equals consistency, when C' equals A plus A', over B plus C.

An effort has been made to adapt this form of Attention test to the games of Foot-Ball and Basket Ball and also to eliminate some of the objectionable features when given according to the instructions in Whipple's 'Manual.' First of all the figures 1, 2, and 3 were arranged in different orders and styled 'Signal A' 1, 2, 3; 'Signal B' 2, 3, 1; and 'Signal C' 3, 1, 2. These signals were first explained and memorized after which they were called out in various orders by the instructor for practice. Then in order to insure against loss of time in case some one should fail to be able to recall the proper signal when given in the test, the subject was allowed to write the signals at the top of the record sheet. In order to eliminate as far as possible the element of immediate memory, which is offered as an objection by Professor Whipple, the numbers were left uncovered after each successive addition, instead of being covered immediately as directed in the manual. When all was fully explained and comprehended by the subject, the instructor announced (just as is done in the game) 'Signal!' (which always means, Attention!) then 'A!,' at which the subject quickly turned his paper right side up and proceeded to add 'Signal A,' *i. e.*, 1, 2, 3, respectively to the numbers found printed at the top of the columns on the record sheet. This was kept up until the instructor announced another signal, whereupon the subject proceeded immediately to add this signal, and so on until the test was completed as mentioned above.

It is believed that this adaptation of the simultaneous adding test will practically eliminate the element of immediate memory and thus provide a test which will measure directly the ability to concentrate attention. It will not, however, serve as an absolute test of the ability of different individuals to concentrate Attention, since the number of additions that any individual performs is not in itself an index of such ability. On the other hand, it does seem reasonable that the difference in the number of additions performed by the same individual at different trials may be attributed to the difference in his concentration of attention, hence the trait of degree of attention in a given individual may be measured in a comparative way by the simultaneous adding test, when the element of immediate memory is eliminated.

Table IX

Test No. 41

Table Showing Record of All the Subjects in Respect to *Suggestibility*, as Measured by the Progressive Weights Illusion

No.	(1) At the Beginning of the Study						(2) At the Close of the Study					
	A	*B*	*C*	*A'*	*S*	*S'*	*A*	*B*	*C*	*A'*	*S*	*S'*
1	7.4	5.2	1.4	.44	20	85.91	8.8	4.4	.8	.48	20	88.75
2	6.8	5.4	1.8	.43	20	88.11	5.9	6.3	1.8	.38	20	80.79
3	6.2	5.4	2.4	.43	20	85.11	7.4	3	3.6	.55	20	92.73
4	9.8	3	1.2	.55	20	92.73	10	4	0	.50	20	90
5	8.8	4.6	.6	.47	20	88.09	8.6	4.6	.8	.47	20	88.09
6	8	4.6	1.4	.47	20	88.09	8.2	4.4	1.4	.48	20	88.75
7	12.8	1.2	0	.62	20	96.45	6	7.4	.6	.33	20	74.55
8	10.8	3.2	0	.54	20	92.23	11.6	2.4	0	.58	20	94.14
9	10	3.2	.8	.54	20	92.23	9.8	3.4	.8	.53	20	91.70
10	7	6.8	.2	.36	20	78.33	5.8	7.6	.6	.32	20	73.13
11	7.2	5.2	1.6	.44	20	85.91	7.8	4.8	1.4	.46	20	87.39
12	6.2	6.6	1.2	.37	20	79.56	6.2	6.6	1.2	.37	20	79.56

Suggestibility, as determined by progressive weights illusion. Standard set of fifteen weights used. Results based upon five trials each hand. Own formula devised for evaluating data, as follows: X equals weight of first stimulation weight. Y equals the weight of the last stimulation weight. Z equals the number of stimulation weights, S equals the force of the stimulation, when S equals Y over X, times Z, minus Z.

It is believed that this formula will be found reliable for estimating the force of the stimulation used in tests of this kind even when the ratio of the stimulation weights, or the number of stimulation weights, varies from that used in this test, *e. g.*, if the stimulation weights were given as 20, 40, 80, 160, 320, the force of the stimulation would figure 75. If the series were given as 20, 40, 60, 80, 100, 120, 140, the force of the stimulation would figure as 42; whereas in the test as given the series is 20, 40, 60, 80, 100, hence the force of the stimulation figures 20.

Having thus determined the force of the stimulation, which of course remains constant throughout the experiment, the following formula has been devised for further evaluating the data: A equals the average number of plusses. B equals the average number of equals. C equals the average number of minuses. A' equals the index of coefficiency, when A' equals A plus C, over S. S' equals suggestibility, when S' equals 100 minus 1 plus B, over A.

Below is given a somewhat more elaborate formulation of this method, which is based upon the following theory of reaction. First, when any series of stimulation, as that of progressive weights, is applied, the effect of same upon the subject is shown by a major wave of reaction followed by several minor waves. Second, the major wave is represented by the number of consecutive plusses that follows the last stimulation weight, while the minor waves are represented by all the plusses and minuses that follow in the series subsequent to the end of the major wave. Third, the amount of resistance offered by the subject is represented by the number of equals recorded subsequent to the last stimulation weight. The formula based upon the above theory is as follows: A equals the number of consecutive plusses beyond the last stimulation weight. B equals the number of equals subsequent to the last stimulation weight. A' equals the number of plusses beyond A. C equals the number of minuses beyond A. S equals the force of the stimulation (determined by the formula given above). E equals the index of coefficiency, when E equals A over S. E' equals the minor index of coefficiency, when E' equals A' plus C, over 1 plus B. S' equals suggestibility, when S' equals 100 minus E times (E' plus 1).

Table X

Test No. 42

Table Showing Record of All the Subjects in Respect to *Suggestibility*, as Measured by the Progressive Lines Illusion

No.	(1) At the Beginning of the Study				(2) At the Close of the Study			
	S	*A*	*B*	*S*	*S*	*A*	*B*	*S*
1	20	49.8	114.4	38.1	20	33.5	293	97.7
2	20	49.5	160.4	53.4	20	49	205.2	68.4
3	20	51.9	70.6	23.5	20	47.5	163	54.3
4	20	54.5	118.5	39.3	20	45.5	64	21.3
5	20	55	94.3	30.1	20	40	16.7	5.4
6	20	51	163.6	54.5	20	38.5	176	58.7
7	20	45.5	108.5	36.2	20	49	64.7	24.4
8	20	54.0	80.4	26.8	20	53	83.5	27.8
9	20	45	105.2	35.1	20	50	111.5	37.1
10	20	58	231.7	77.2	20	58.5	30.5	10.2
11	20	60	74.3	24.8	20	55	237.7	79.2
12	20	45	161.7	53.9	20	45	161.7	53.9

Suggestibility, as determined by progressive lines illusion. Standard set of lines used. Records made on graph paper. Results based upon two trials through exposure of 21 lines beyond the last stimulation line. Own formula devised for evaluating data, as follows: X equals length of first stimulation line. Y equals the length of the last stimulation line. Z equals the number of stimulation lines. S equals the force of the stimulation, when S equals Y over X, times Z, minus Z. A equals estimated length, by the subject, of the last stimulation line. B equals the aggregate error in millimeters both ways, from the standard A, made by the subject in the entire series subsequent to the last simulation line. S' equals suggestibility, when S' equals the term sought in the equation Y over S equals B over S'.

The charts here mentioned appear on pp. 357, 358 and 359 and are properly labelled there.

Results Obtained from the Experiment

During the three months covering the period of the study the following changes took place in the *motor reaction*, *attention* and *suggestibility* of the subjects under consideration, as determined by the ten different tests employed.

The *practice* group of men lost 14.9 per cent. in *motor reaction*, gained 10.8 per cent. in *attention* and gained 29.6 per cent. in *suggestibility*.

The *non-practice* group of men lost 1.3 per cent. in *motor reaction*, lost 4.4 per cent. in *attention* and lost 10.3 per cent. in *suggestibility*.

The *practice* group of women gained .7 per cent. in *motor reaction*, gained 7 per cent. in *attention* and lost 7.7 per cent. in *suggestibility*.

The *non-practice* group of women gained 8.3 per cent. in *motor reaction*, lost 4.9 per cent. in *attention* and lost 4.1 per cent. in *suggestibility*.

The *practice* men and women taken together lost 6.1 per cent. in *motor reaction*, gained 8.8 per cent. in *attention* and gained 10.9 per cent. in *suggestibility*.

The *non-practice* men and women taken together gained 2.2 per cent. in *motor reaction*, lost 4.7 per cent. in *attention* and lost 7.2 per cent. in *suggestibility*.

All the men taken together lost 6.9 per cent. in *motor reaction*, gained 2.6 per cent. in *attention* and gained 9.3 per cent. in *suggestibility*.

All the women taken together gained 3.8 per cent. in *motor reaction*, gained .6 per cent. in *attention* and lost .7 per cent. in *suggestibility*.

The highest individual record at the beginning of the study was made by men in *four* and by women in *six* of the ten tests.

The highest individual record at the close of the study was also made by men in *four* and by women in *six* of the ten tests.

The lowest individual record at the beginning of the study was made by men in *six* and by women in *four* of the ten tests.

The lowest individual record at the close of the study was made by men in *eight* and by women in *two* of the ten tests.

Pooling all the ten tests given both at the beginning and

at the close of the study and taking this as a basis for measuring the abilities of the subjects in respect to *motor reaction*, *attention* and *suggestibility* all taken together, the *non-practice* men appear 8.2 per cent. stronger than do the *practice* men. The *non-practice* women appear 7.5 per cent. stronger than do the *practice* women. The *non-practice* men and women taken together appear 7.2 per cent. stronger than do the *practice* men and women. All the *women* taken together appear 12.2 per cent. stronger than do the *men*.

Of course in these last comparisons based upon the pooling of all the tests it is to be remembered that *stronger* means a faster rate of, and more steadiness of, *motor control* and a greater degree of ability to concentrate *attention*, but it also means a greater degree of susceptibility to influence through *suggestion*.

Some Conclusions Drawn from the Foregoing Study

In a general way the study seems to warrant, in a measure at least, the following conclusions, to wit:

The persistent practice of basket ball for a season of three months has the tendency to break up control of *motor reaction* by reducing the *rate* of voluntary movement and rendering the subject *less steady* in point of involuntary movement. But at the same time such exercise has the tendency to *increase* the subject's power to concentrate *attention*, as well as to render him *more susceptible* to influence through *suggestion*.

Considered on the basis of sex, the persistent practice of basket ball appears to affect *men*, in respect to *motor reaction*, *negatively* MORE than it does *women*. In respect to *attention*, the *men* are affected, *positively*, MORE than are the *women*. In respect to *suggestibility*, the *men* are affected, *positively*, VERY MUCH MORE than are the *women*.

This difference in the degree of effect shown upon the *men* when compared with the *women* is no doubt partly due to the fact that as a rule men play the game much harder than do the women. This fact is obvious for two reasons, first, we may say, because of the natural difference in the activity of men and women and second because of the limitations set upon the

activity of women by the restrictive rules for women's basket ball games.

It should be said with reference to the conclusions herein stated that they are rendered rather more conservative than they might otherwise have been by the fact that one of the non-practice women, namely, subject No. 12, was obliged to drop out on account of illness, hence her record for the close of the study being wanting was necessarily supplied. Since this was done by duplicating her record made at the beginning of the study, it would tend to reduce the differences shown between the *practice* and the *non-practice* women, assuming of course that this subject would have shown changes in a general way similar to the other two members of the non-practice group.

As measured by the tests used in this experiment it appears that *men* are superior in respect to *motor reaction*, while the *women* are superior in point of *attention*, and less *suggestible* than are the men.

Assuming that the average person is quite *suggestible* enough, it would appear that persistent practice at basket ball is *un*desirable.

Again, assuming that increased ability to concentrate *attention* is to be desired, it would appear that persistent practice at basket ball *is* desirable.

Finally, assuming that it is *un*desirable to be broken up in point of *motor control*, we would have two points against, with only one in favor of, basket ball as a form of athletics to be recommended to the average college student.

PSYCHOLOGICAL TESTS AS APPLIED TO THE CRIMINAL WOMEN

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The following is the experimental data presented in a twelve-minute paper at the American Psychological Association in December, 1912. The work was begun in July, 1911, under a grant from the New York Foundation and is being carried on at present by the Bureau of Social Hygiene which has established the Laboratory of Social Hygiene at Bedford Hills in affiliation with the New York State Reformatory for Women. The scheme includes ultimately an exhaustive study of the mental and physical history and condition of a large number of criminal women with the hope that, among other things, once the research is ended and tests established, the State of New York will see fit to include in its machinery for dealing with the criminal women a clearance house to which she may be sent after being judged guilty, and from which after examination she may be sentenced more wisely in accordance with her possibilities and needs and not arbitrarily according to her crime. The problem that was in mind in the application of the tests herein reported was a first step toward the formation of a body of tests which could be applied after a woman's conviction and preceding her sentence which should prove a safe guide as to her reformability.

The women sent to us vary in age from sixteen to thirty. The average physical age of the first thousand inmates committed to Bedford was 20.8 years. The average age of the two hundred women reported upon in this paper is 20.47 years.

Upon the apparently universal assumption that the feeble-minded are not reformable nor able to conduct themselves wisely outside of an institution, and because an appreciable per cent. of our girls were obviously feeble-minded, we felt it desirable to determine exactly what this per cent. might be.

For this purpose Dr. Goddard's adaptation of the 1908 Binet series was at hand. Although skeptical of their absolute clinical value for mature women, it seemed advisable to test their serviceableness; for, at the time this work was begun (July, 1911) none of the more recent critical work of the Binet tests had been published. Accordingly the writer gave these tests under the most precise of laboratory conditions to two hundred women as they came to us in sequence from the courts, to find that only .5 per cent. could pass the 12-year-old test and that the average mental age was 10.07 (7.785) years.

2 tested	6 years
15 tested	7 years
13 tested	8 years
47 tested	9 years
74 tested	10 years
48 tested	11 years
1 tested	12 years

Inasmuch as a fair per cent. of this number were bound to prove reformable if the practical experience of the institution was to be relied upon, we must needs conclude either that the feeble-minded were reformable or that the tests, at least as applied to the criminal woman, were inadequate.

Moreover, we have since tested, with the aid of Dr. Mabel R. Fernald, twenty-five girls in the Chicago Normal School (A, B and C students in psychology) and of these, too, not a few failed to pass some proportion of the 9-, 10-, 11- and 12-year-old tests, and have an average age of 11.49 (9.18) years.

In addition, I have given these tests to a group of the more intelligent maids at the University of Chicago and at Vassar College, selecting those who had worked steadily and efficiently in one or the other of these places for at least five years, to find that they average only 10.75 (8.2) years by these tests.[1]

Obviously Goddard's adaptation of the Binet tests, in which both may often test the same, does not serve to differentiate the efficient working woman from the feeble-minded girl of 10, 11 or 12 mental years. Indeed we are convinced that general intelligence tests, such for instance as the Binet test,

[1] There is in preparation for the press a detailed account of these three groups in their reaction to the Binet tests.

may be interesting as a general index of mental training, but are much less useful among the women of whom I speak than motor coördination tests. The Binet tests fail on the one hand because there are a fair proportion of our inmates who are not mentally inefficient but mentally inert. Their lives and minds are so constituted that they feel no need to learn the things 'any child ought to know' though they can and do learn when we teach them. Take for example such a test as Binet's definition of the three abstract terms, 'charity,' 'justice' and 'kindness.' If two of the three are not defined by the child of 12 it may be fairly indicative that he is unable to handle abstract terms. Of our group while only 35 per cent. know the meaning of 'charity' and 44 per cent. say they have never heard the word 'justice,' 96 per cent. can define adequately such words as 'kindness,' 'meanness' and 'goodness.' They will tell you for instance that goodness is a matter of character. It is obviously unfair to assume that because they cannot define two abstract terms the power to think abstractly is denied them.

On the other hand, these general intelligence tests fail to make out as subnormal certain girls of whom we have not a few, who in general information are entirely normal but who are otherwise constitutionally unfit, whose voluntary control is poor, who are easily distracted and emotionally unstable.

The tests that may better be expected to separate the stable, reliable woman from the irreformable one are those that simulate the jobs at which they must earn their livings,—that are simple enough as to directions so the dullest girl can follow them, that demand continuous and continued attention and some patience and nervous resistance.

Such a test, for instance, is the one reported by Bogardus in the *American Journal of Sociology* in 1911.[1] With an apparatus which imitates amazingly well in miniature the average dangerous factory machine, he tested the relation of fatigue to industrial accidents. We have duplicated the test here to find that where the more dependable woman whom the institution

[1] Bogardus, Emory S., 'The Relation of Fatigue to Industrial Accidents,' *The American Journal of Sociology*, Vol. XVII., Nos. 2, 3 and 4.

selects as the reformable one approximates the normal record, making two thirds as many errors in the second half of the working period as in the first and coming fairly near the normal average of errors (3.87 first half and 8.28 second half), the irreformable girl may make as many as 1,335 errors in the first half and 312 in the second half. They work so inaccurately that the errors from fatigue are completely lost sight of. Each day they have to re-learn the 'operation,' so that more errors are apt to come in the first and less in the second half of the working period. Or, again, they are quite as likely to slowly establish bad habits as to laboriously form good ones, and thus the number of errors per day may increase rather than decrease,—due in no sense to fatigue but to practice!

In general, the mental characteristic most peculiar to the criminal woman as she exists as a type at Bedford is an inexplicable narrowness of her scope of interest. What they know may be in quality entirely mature; in quantity it is almost certain in a very high majority of cases to be but a fragment of the mental content of a normal person of average intelligence. If the quality be normal, the 'quantity' is a matter contingent largely upon the character and persistence of previous experience and the capacity for new habit formation. No problem that is set is more important and nothing more difficult than to establish a means of solving with dispatch in advance how the balance lies between these two 'contingent' factors.

Another interesting characteristic of the majority of these women is their slow and variable reaction time. When tested with the Vernier Chronoscope their visual reaction time is .35 secs.; the auditory reaction time is .20 secs. This superiority of audition is borne out by the fact that they follow spoken directions better than written ones. The mean variation of the reaction time of a normal person after practice is about one tenth of his average reaction time. The mean variation for the criminal woman is seldom less than one fifth of her average reaction time.

This slowness of reaction time is shown both in tests of

motor coördination and association of ideas. When, for instance, they are asked to give the verbal opposite of ten easy words, only .906 per cent. come within the slowest time (1.50 seconds) given by Woodward's subjects in response to his series of easy opposites and only .011 per cent. come within the best time of his subjects (1.03 seconds); only 24.62 per cent. make as few as one error or less; 60 per cent. make more than one error but less than six errors and failures taken together, 15.38 per cent. as many as five or more failures. (A failure all wrong; an error a response that is partly right.)

If asked to say as many words as they can in three minutes, only 47 per cent. can succeed in saying as many as 60.

With the Jastrow card-sorting apparatus, only one out of two hundred girls comes within the slowest time made by the twenty-five university girls tested by Dr. Thompson.[1] To measure their ability to learn a simple motor coördination by five trials of tracing a star in a mirror, is to find that whereas twenty-three college women tested by Whipple take 127 seconds for the first and 67 seconds for the last trial, a consecutive series of fifty of our girls take on the average of 485.72 seconds for the first trial and 117.53 seconds for the last trial; also that whereas his women make 34 errors the first trial, ours make 190.08 with 46.15 errors in the last trial.

Only 9 per cent. can repeat more than seven numbers after hearing them spoken; 47.5 per cent. can repeat seven numbers; 28.5 per cent. only six and 25 per cent. less than six. Only 11.5 per cent. can repeat after one a sentence containing 26 syllables; 81.8 per cent. are unable to repeat a sentence containing 24 syllables.

After reading a passage containing 25 unit words and phrases, 79.87 per cent. remember less than half, and 54.54 per cent. remember less than 10 units. This passage can be read comfortably in 14 seconds. The best 11.5 per cent. of our women take on the average 15.9 seconds to read it. 12.5 per cent. cannot read in any language and 11 per cent. cannot write in any language.

[1] Thompson, Helen B., 'Psychological Norms in Men and Women,' The University of Chicago Contributions to Philosophy, Vol. IV., No. 1.

These delayed reaction times; this narrowness of memory span; this failure to sort cards and learn to draw stars in the mirror, etc., as well as college women can do them, may to a certain extent be true also of the normal [efficient + law-abiding] working girl and woman. At least until we have tested these latter we cannot say that our women are subnormal. The possibility is that many of them are; but the chances are likewise very great indeed that the records of the better 40 per cent. of the criminal women will be in most respects like the normal working women's, and that both will be appreciably below those of the college girl in quantity and rate—if not in quality.

Our most imperative need in the field of mental tests as applied to the criminal woman is the determination of norms, the determination, at least, of the lowest and the average degree of intellectual capacity and motor control which a law-abiding woman must possess to earn a living.

In conclusion, it may be of incidental interest to add that none of these women are found to be color-blind; that 5.6 per cent. are left-handed, 3 per cent. are ambidextrous, 3 per cent. tattooed, 4 per cent. unmistakably insane, and 15 per cent. addicted to morphine or other drugs.

THE FUNCTION OF INCIPIENT MOTOR PROCESSES

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The construction of an hypothesis can never give the same repose and satisfaction to the scientific conscience as does the discovery of even a moderately significant fact. And hypothesis-building brings with it the less sense of solid accomplishment, the more its results are removed from the actual test of fact. Hence the person who tries to erect a theory as to what occurs in the nervous system, in connection with any processes less amenable to experimental conditions than scratch reflexes, is not likely to feel a gratification proportionate to his pains. And yet a consistent theory of the physiological processes underlying the higher mental processes would be of some practical value, even though it could not be tested by physiological experiment. For a coherent presentation of the facts demands some principle of explanation, and the laws of learning, which lie at the bottom of the complexer processes of mind, suggest certain physiological assumptions which logically demand to be followed out and elaborated into a complete physiological theory.

The present paper aims to suggest an hypothesis regarding the nature of some essential features in the nervous process underlying the production of a mental image, a revived or centrally excited mental process. Since the leading part in such processes, according to this theory, is played by the initiation of movements that are not fully carried out, it may be termed *the theory of incipient motor processes.* As a preliminary to consideration of the image, we may discuss the physiological nature of the associative processes in general.

There are grounds for thinking that to that form of association which has traditionally been called the association of ideas, the association of movements is preliminary. In the so-called association of ideas we have called into consciousness

the image of a past experience. If stimulus *A* and stimulus *B* have at some former time been experienced together, the occurrence of *A* 'makes us think of' *B*; that is, calls up a mental image or centrally excited sensation of *B*. There are two reasons, at least, for believing that a more primitive process than this is to be found in the type of association whereby stimulus *A* comes to produce the *movement* which formerly resulted from stimulus *B*, rather than an *image* of stimulus *B*. These two reasons are as follows. First, among the lower animals we have constant proof that one stimulus may through being repeatedly experienced in connection with another, come to assume the motor tendencies of the other; but we have very limited evidence of the occurrence in the animal mind of the associative production of images. Secondly, in the human mind, the association of images rests absolutely upon attention. Not the fact that the stimuli *A* and *B* occur together gives to *A* the power of calling up an image of *B*, but the fact that the two are attended to together. And whatever else attention may mean, the fact is reasonably certain that simultaneous attention to two things means a simultaneous motor response to them. The dependence of association upon attention, an essentially motor phenomenon, becomes comprehensible if we think of association as being itself primitively an association of movements.

Let us then first consider the processes by which stimuli come to be associated with new movements. We shall use in this consideration four fundamental physiological assumptions, all of which have some warrant from the facts discovered in experiments on simple reflexes.

1. When a motor response is initiated, all the sensory centers[1] that are receiving stimulation at the same time contribute to that response a part of their energy. This is the familiar fact of *Bahnung*, as illustrated by Exner's case of a sound stimulus reinforcing a touch stimulus in producing movement of a rabbit's foot, or by Yerkes's demonstration of the effect of sound stimuli on the reaction of the frog to

[1] I shall use throughout this paper, for convenience, the rather old-fashioned terms sensory and motor *centers*, leaving undetermined whether a center involves one or many neurones.

electrical stimuli.[1] While the principle of Bahnung is usually stated to hold true of certain allied systems of reflexes, we have stated it as a perfectly general principle, with the understanding of course that it may be crossed by other principles.

2. Whenever a sensory center has any part of its energy drained into a motor outlet, the resistances along the path leading to that outlet are decreased. It is unnecessary to dwell upon this assumption. In one form or another, it is indispensable to any theory of the more complex workings of the nervous system.

3. One motor response may be prepotent over others tending to occur at the same time. This prepotency consists in the fact that certain motor responses will be called forth by a much slighter intensity of stimulus than others. For this statement we have the authority of the experimental physiologists, and Sherrington has emphasized the fact that the prepotent responses are those most concerned with welfare.[2]

4. There exist antagonistic motor responses, so connected that the making of one inhibits the making of the other. This statement, also, needs no defence: it is a well-established fact.

To proceed now with our investigation of the process whereby the association of motor processes is brought about, let us suppose two stimuli, *A* and *B*, acting simultaneously upon the organism, and let us suppose further that *A* naturally gives rise to the response *AR* and *B* to the response *BR*. We may further assume that these two responses, *AR* and *BR*, are not antagonistic to each other; that is, that the organism can carry out both movements at once. Now if we have recourse to assumption 1, that when any motor response is made, a portion of the energy of every stimulus acting on the organism at the time is contributed to the making of it, we may assert that a part of the energy of *A* goes to the production of *BR* and a part of the energy of *B* goes to the making of *AR*. If we call assumption 2 also to our aid, whereby the oftener a nervous process travels a certain

[1] See Sherrington, 'The Integrative Action of the Nervous System,' p. 175.

[2] *Op. cit.*, pp. 228–231.

pathway the less the resistance to its passage, we see how the frequent occurrence of A and B together can give to either A or B the power of initiating the combined reaction AR–BR, without the actual occurrence of the other stimulus. This we may call Type I of motor associations.

In other cases it happens that AR and BR are not compatible but antagonistic reactions. Now a billiard ball (if I may be pardoned for using that well-worn object as an illustration), when acted upon by two forces in opposite directions is influenced fully by both of them. The weaker loses none of its effect because a stronger one is on the field: it is able to diminish the action of the stronger by the full amount of its own strength. This would be an exceedingly inconvenient principle to govern the actions of a living organism: hence the nervous system works in such a way that the stronger stimulus can wholly suppress and inhibit the movement that would result from the weaker if it acted alone (Assumption 4). This means that if a weak stimulus A and a stronger stimulus B act together upon the organism, and would if they acted alone demand of it incompatible responses, A finds itself quite cut off from its own motor outlet when the motor paths of the response BR are open. Hence apparently the whole energy of A will be available to find its way into the channels of the response BR, according to assumption 1. We might expect that a pathway from A to BR would be formed with especial rapidity when BR and A's original response AR are antagonistic. But whatever advantage is derived from the fact that in such a case all A's energies are free to take part in the new reaction is probably compensated for by the fact that the resistances which the energy of A has to overcome in finding its way to the antagonistic outlet AR are higher than those encountered on the way to a compatible motor response. Indeed it may well be that a part of the very process by which the reaction AR inhibits BR involves a heightening of the resistances along the paths that would connect AR with a stimulus belonging to an antagonistic response. But of course it constantly happens that a stimulus does come to be connected with a reaction the very opposite

of that which originally pertained to it. A tamed animal learns to run to the human being from whom it at first fled in terror. In order that the tendency thus acquired for *A* to produce the response *BR* may be permanent, and may displace its original tendency to produce *AR*, either *B* must occur more frequently with *A* than *A* occurs alone, so that the pathway from *A* to *BR* becomes more permeable with repeated traversing (Assumption 2), or else the response *BR* must be a prepotent response, one that is specially ready to occur provided that even a weak current of excitation reach it (Assumption 4). Such a prepotent response is the negative reaction following harm to the organism, and hence we find that this response readily becomes associated with stimuli that are not in themselves harmful but have been accompanied by injurious stimuli. The type of learning where a stimulus *A* comes to produce a motor response *BR*, opposite in character to its original response *AR*, because of the experiencing of *A* and *B* together, we may call Type II.

Now thus far we have been considering the cases where the two stimuli *A* and *B* act simultaneously upon the organism. But a stimulus *A* may come to produce a response *BR* which originally pertained to another stimulus *B*, when *B* has occurred not together with *A* but immediately after it. The simplest case of this sort will happen when *B* intervenes after the reaction *AR* has been started, but before it has been completed. If *A* is to become associated with the new response *BR*, the latter must draw off into its own channels some of the energy of *A*, and prevent its all being discharged into *AR*. The more natural effect in this case would seem to be for the later stimulus *B* to have all its energy diverted into the already active channels of *AR*, according to Assumption I. To explain how the later response can ever drive the earlier one from the field, we must suppose the later one to be a prepotent response, so ready to occur that even slight excitation will produce it. When *AR* and *BR* are not incompatible reactions, what happens may be simply that *AR* is initiated by the occurrence of *A;* but *B* immediately following and opening the prepotent path *BR*, a part of *A*'s

energy is diverted into *BR*. The oftener this happens, supposing *BR* to be a prepotent response, the slighter is the tendency to produce *AR* at all; thus we have the gradual dropping off of movements which while they are not really antagonistic to the prepotent response, are simply unnecessary, such as the random movements that accompany early attempts at writing or skating, or the aimless struggles of an animal learning to get out of a puzzle box. When, on the other hand, *AR* and *BR* are really antagonistic, we have to suppose that the reaction *BR*, breaking in on the already initiated *AR*, inhibits it not gradually, by depriving it of a part of the energy of its stimulus, but automatically and at once. Thus all the remaining energy of *A* has to find its way into the channels of *BR*. The only difference, it would appear, between these cases, which we may call Types III and IV, where the stimuli are successive and those (I and II), previously considered, where they are simultaneous, is that greater prepotency of one response over the other is demanded for Types III and IV; of the later response over the earlier one, to counteract the advantage of the earlier one in having already got itself started.

Evidently the highest degree of prepotency is required of a reaction that can supplant another which has not only been initiated first, but has actually been carried out. Yet this supplanting does occur in many cases of learning. A stimulus *A* produces its full response *AR:* there follows upon this response a stimulus *B* with its appropriate reaction *BR*. How can *A*, whose energy was wholly used in producing the reaction *AR*, contribute anything to the production of *BR*? The only answer to this question, so far as I can see, lies in the formulation of a further physiological assumption, namely: (5) A certain portion of the energy of a stimulus is left behind after its motor response has occurred, and may be drained into the channels of the next-following motor response. Now for the connection between *A* and *BR*, thus formed through the slight residue of *A* left after the response *AR* has occurred, to become victorious in subsequent experiences over the original response *AR*, and bring it about

that *A* produces *BR* instead of *AR*, it is necessary that *BR* shall have a high degree of prepotency over *AR*. The reaction *BR* in such cases is probably always of the type called by Sherrington 'consummatory reactions,' whose performance is intimately connected with the immediate welfare of the organism. We may call the types of learning which involve the supplanting by another movement of a movement that has actually been carried out, including the two cases, where the movements are antagonistic and where they are not, Types V and VI.

None of these six types of association needs to involve the production of an image or centrally excited mental process, in which the sensory effects of past stimulation are revived. To account for the origin of the image, I should like to propose another physiological assumption, the sixth. It is this: (6) When the cortex has reached a certain degree of development, if a motor response is initiated, all the sensory centers that have recently or frequently discharged into the motor center concerned, that is, all the sensory centers with low resistances at the synapses along the pathways leading from them to the motor center, are set into excitation. This excitation is more marked, and more wide-spread, the greater the delay between the initiation and the execution of the movement. That is, while with a short delay activity will be induced in those sensory centers most closely connected with the motor center concerned, a longer delay will cause the spreading of the induced activity into more remotely connected sensory centers. Upon the activity thus induced in sensory centers, whether near or remote, the image or centrally excited conscious process is based.

The common conception of the process whereby sensory centers are centrally excited, giving rise to images, is that nervous activity passes from one sensory center to another, along a nervous pathway whose resistances have been lowered by the former simultaneous activity of the two centers. Thus there is supposed to be an actual transfer of energy along an associative pathway, just as in the peripheral excitation of a center a transfer of the stimulus energy occurs

along a sensory pathway. The view here proposed maintains, on the other hand, that the discharge of a 'centrally excited' center does not occur by the influx of any energy into it from without. Our theory suggests rather that a discharge of the stored-up energies of the sensory center into a motor center is brought about when the motor center in question is partially excited from another source and the resistances are already low between it and the sensory center whose discharge is thus induced. The process of central excitation would be thus *induced* and not *transferred:* the disturbance of equilibrium in the motor centers draws the stored-up energies from the sensory centers associated with them. It is as if a hitch in the functioning of a motor center enabled it to call to its aid contributions from all the sensory centers connected with it by paths of low resistance. Thus, for example, if in Type I of learning, where the stimulus *A* has become able to produce both its own original response *AR* and the response *BR* which originally pertained to stimulus *B*, the response *BR* is only partly initiated, there will be, supposing the cortex of the animal to have reached the proper degree of development, an induced activity in the sensory center formerly affected by stimulus *B*, with the result in consciousness of an image of *B*.

This theory demands, not that the partial excitation of the motor center *M* shall send a nervous process *to* the sensory centers associated with *M*, which would involve either violating the principle of the irreversibility of synapses or the assumption of a double conduction path; but that the partial excitation of *M* in some way draws a current of excitation down from the associated sensory centers into *M*. While no clear physical or electrical analogy for a process of this sort can be found, we at least contradict no known fact of nervous action by supposing that the excitation of *M* lowers resistances at all the synapses of *M*. This sudden lowering of resistances may be regarded as giving an opportunity for the associated sensory neurones to discharge into *M*. Such an action is assumed by MacDougall to occur in the case of the reciprocal innervation of antagonistic muscles.[1] But of

[1] *Brain*, vol. 102, page 153.

course to explain the discharge of sensory neurones without external stimulus, we must assume in them a tendency to discharge as soon as there is a sudden reduction in the synapses connecting them with the motor area; a kind of unstable equilibrium. Further, we need to understand wherein that degree of cortical evolution consists which makes the mental image possible. For certainly it seems that frequency and free functioning of images is peculiar to minds of the highest order of development, and is much less marked in those of even the highest vertebrates below man. May we not then suppose that the distinguishing characteristic of a cortex sufficiently highly evolved to undergo processes that are accompanied by centrally excited mental processes or images, is a high degree of instability, tension, or potential energy in its sensory centers, such that their discharge will occur 'spontaneously' whenever there is a sudden lowering of resistances at their synapses? Our seventh physiological assumption would then be: (7) In a highly evolved cortex, sensory neurones are in a state of unstable equilibrium and readiness to discharge, such that a suddenly lowered resistance at any of their synapses may induce their discharge into a motor pathway.

It is now high time to consider the arguments which favor making this formidable array of assumptions. The order in which we discuss them is not especially important; we may begin with one that is not particularly weighty, namely, that introspection seems to reveal to us a practical function for mental images, such as that we have described. For instance, take the case of a man shut up in a room from which he has previously released himself by working a combination lock. His glance falls upon the lock: the external stimulus sets up a tendency to move, which however needs the help of memory images before it can be fully executed. "There's the lock; now what were the turns I had to make?" he asks himself: the partial initiation of the response calls to its aid centrally excited processes, and the movements are successfully performed under the joint incitement of peripherally and centrally excited currents.

This, however, is an illustration rather than a real argument. The consideration which first suggested to the writer the necessity of some such physiological theory of the image as that here described was, as has been previously intimated, the fact that the association of A and B which enables A to call up an image of B does not rest merely on the simultaneous occurrence of the stimuli A and B on some previous occasion. We associate A and B only if besides being experienced together they have been attended to together. Now there are only two ways in which the necessity of simultaneous attention to A and B can be interpreted. First, the physiological changes underlying attention to A or B may be supposed to affect the sensory processes resulting from the action of the stimulus A or B. Or secondly, they may be thought of as involving characteristic motor reactions to the stimulus A or B. In a word, attention must be influential upon association either through a sensory or a motor effect. One can hardly conceive any influence of attention upon sensory processes alone which does not reduce itself to an increase of the intensity of such sensory processes. It may be said, for example, that attention, bringing about a better reception of the peripheral stimulus and a reinforcement of it by centrally excited processes, makes the sensory processes resulting from the stimuli A and B more intense, and that such intensity is necessary to bring about their association. But it would be hard to show, on such a hypothesis, why an increase of intensity that did not result from attention, but from increase in the physical force of the stimulus, should not be equally effective for the formation of associations. We know, however, that mere intensity of stimulation, apart from attention, has no significance for association. It would seem, then, as though the essential dependence of association on attention must rest on the essential motor character of association.

A second argument in favor of this theory is that it offers a convenient and clear way of conceiving the relation between imagery and learning, and of the *anschaulich* to the *unanschaulich* accompaniments of learning. It is evident that as motor processes become more completely organized, and

learning is complete, there occur *pari passu* an increase of the speed with which the movements are performed and a decrease in the amount of imagery present. It thus seems natural to connect the presence of imagery with delay and hesitation in motor response. In order, further, to understand the stages which introspection reveals in this process of the gradual disappearance of imagery, we need to note a further peculiarity which characterizes the complexer cases of learning.

This peculiarity consists in the fact that in higher learning processes we have the formation of *movement systems*. Now movement systems are characterized by the fact that when motor responses are associated, one response does not supplant another, but the performance of one response is rather an essential condition for the performance of another. The practical importance of the motor responses depends upon their all being actually made: one movement in the system is of no use without the rest. The conditions are such that there cannot be established a short-cut through the elimination of certain movements altogether: the movements derive their value each from the actual performance of the other. It is unnecessary to point out how frequently learning has to take this form. Most movements, in fact, are not simple but complex, and consist of the performance of a number of motor responses each of which would be useless without the others.

Of course such connections between motor innervations are in many cases innate. But there are others which are acquired during the lifetime of the individual. Now the most natural way in which we may suppose such movement systems to be learned or acquired is by some arrangement through which the performance of one movement may itself furnish the stimulus, or a part of the stimulus, for another movement. And the most obvious method by which the performance of one movement may regularly provide the stimulus for another is by the processes which are set up in sensory pathways by the action of the muscles themselves. Acquired systematic connections between movements are

most naturally explained by supposing the dependence of one movement in a system on the kinæsthetic or proprioceptive excitations resulting from the performance of another movement as a part of its stimulus. If reaction *BR*, to which *B* is the appropriate stimulus, is also dependent on the occurrence of the kinaesthetic excitation *KAR*, resulting from the performance of reaction *AR* to stimulus *A*, we shall have the connection of the two movements *AR* and *BR* into a system. If the connection is simply that *KAR* must combine with *B* to produce *BR*, then the system will be one of successive movements: movement *BR* must be preceded by movement *AR*. But if the connection is mutual, so that the full stimulus to movement *BR* is *KAR plus B*, and the full stimulus to movement *AR* is *KBR plus A*, then each movement demands the performance of the other, and we have a simultaneous system of movements: the connection is made in all directions. Practically all systems of movements, whether they are successive or not, involve simultaneous systems: that is, even a succession of movements is usually a succession of complex movements, or simultaneous systems of movements.

Now while as learning progresses, the tendency is for imagery to disappear and for the movements to be carried out automatically, introspection shows that at a certain stage of this process, while there is no clear visual, auditory, or verbal imagery accompanying the performance of a system of movements, there are present in consciousness certain 'imageless' or *unanschaulich* conscious processes, certain awarenesses or conscious attitudes or thoughts. These we can explain, on our theory of the basis of the image, as kinæsthetic or proprioceptive in their origin, and we can see why they should disappear only at a later stage of learning, or the formation of movement systems, than that at which visual and other *anschaulich* processes vanish. In a complex system of movements, the sensory centers connected with a given motor center are of two orders: first, those corresponding to the original stimulus to the movement, visual, auditory, or whatever it may have been; and second, the various kinæsthetic centers whose excitation results from the performance

of the other movements in the system and forms a part of the proper stimulus for the motor center we are considering. If, then, this motor center is partially excited, and there is a delay in the execution of the motor response, there are two kinds of imagery that may be aroused: the one may be visual, auditory, or in short may belong to any modality; the other must be kinæsthetic and must relate to the system of movements itself. The latter, we may suppose, constitutes the *unanschaulich* conscious accompaniments of the movement system. And since in the formation of a movement system while the actual occurrence of the original external stimuli that belonged to the various movements comes to be eliminated, the actual performance of the movements of the system never comes to be eliminated, because by definition the system cannot afford to drop out any movement, we can see that the kinæsthetic centers would be much more intimately connected with the motor center than would the sensory centers concerned with the stimuli of other modalities which originally appertained to the movements of the system. Since a slight delay in the performance of a movement calls into activity the sensory centers most immediately connected with the motor center concerned, and a longer delay induces activity in more remotely connected sensory centers, we can explain why at a later stage of learning the imagery involved should all have a kinæsthetic basis, while at an earlier stage, involving longer delays, imagery of other sorts should be called up.

It is evident, finally, that this theory with regard to the physiology of the mental image involves some addition to the current theory of attention. We have based our hypothesis about the image mainly on the undoubted fact of the dependence of recall on attention, and on the supposition of the essentially motor nature of attention. Now the ordinary account of the motor aspect of attention describes it as involving two kinds of motor processes: first, those required to hold the body quiet, so that the stimulus shall be received without distraction, and secondly, those which produce adaptation of the sense-organ for the most favorable reception of the stimulus. The motor processes of the first class have

clearly nothing in them that is specific or differentiated according to the individual character of the stimulus. The same quiet position of the body suits attention to any kind of stimulus; is adapted to listening, looking, or thinking. The adjustment of the sense organ has more relation to the peculiar nature of the particular stimulus concerned: it is of course different when the stimulus is visual and when it is auditory; for a visual stimulus it varies with the distance from which the light rays come and the point on the retina which they strike. There is even a difference in the accommodation process according to the wave-length of the light rays, since the focal distance of the lens varies with the color of the light. But for many stimuli the motor processes which relate to the adaptation of the sense organ would not be differentiated: the same adjustment process would suffice for a whole group whose associative connections would yet be very unlike. It seems to the writer of this paper that in addition to the two classes of motor effects of attention mentioned above, a third may well be added, and the statement ventured that attention to a given stimulus involves the initiation, at least, of a motor response that is peculiar to that stimulus alone. This would mean that every sensation that can be discriminated in a fusion, and every group of sensations that can be attended to as a single whole, has connected with it one or more movements which are peculiar to it alone. What, indeed, does discrimination mean if not the performance of specific motor reactions? Where would be the use of consciously distinguishing between two sensations if the two were not to lead to different movements? We need not expect always to find by introspection traces of these specific motor processes involved in attention; yet as illustrations familiar to introspection we may take the slight tendencies to articulate or to vary the tension of the vocal cords which accompany attention to sounds, or the tendencies to eye movement that accompany the visual perception of lines and forms.

This view of attention needs fuller elucidation and defense than can be given it here. Our concern at present is only to

point out the relation of the mantal image to attention, on the theory of incipient motor processes. The initiation of a specific motor response, with attention to a given stimulus, induces activity in whatever sensory centers are most directly connected with the response in question, through the previous occurrence of their own response together with it; and the activity of these sensory centers is accompanied in consciousness by images or centrally excited processes. If the question be raised as to why the motor responses whose association gives rise to images must be those motor responses concerned in attention, and not any motor responses whatever, it may be suggested in reply that motor responses which are in connection with *cortical* sensory centers (and no lower sensory centers need be supposed to possess the degree of instability required as a basis for the image) and which are subject to delay between their initiation and full execution are all of them connected with attention.

The design of this paper, expressed in a sentence, is to point out the possible significance as a factor in the physiology of the higher mental processes, of incipient activity in motor centers. While such activity is not itself accompanied by consciousness, probably, the assumption that it has an influence such as that described above affords a means of understanding how the effect of motor response upon consciousness, obviously so great and significant, may be exerted. The theory here presented renders unnecessary any such hypothesis as that of innervation sensations. The writer presents it in the hope that it may prove worthy of some discussion, and may be in some measure suggestive. In a later paper she hopes to discuss further the view of attention sketched above, and also the nature and functions of those very important movement systems, the bodily attitudes.

DISCUSSION.

THE INHIBITORY FACTOR IN VOLUNTARY MOVEMENT.

Apropos of Langfeld's recent article in this Review (November last) on voluntary movement a few remarks seem not untimely.

In the first place, as every research-report known to the present writer seems to strongly indicate, we are wasting not a little time and query over the matter of imagery in studying voluntary movement. The reason that we are still doing so probably lies in our persistent ignoring of the inevitable action of the habituation-process, for an important part of this process is the sinking into the subconsciousness of movement-sensations, visual and kinesthetic, for every movement not truly voluntary, that is, really new and difficult and, as I think, inhibitory. Langfeld himself bears witness to this in his sixth 'conclusion': "There were subjects who required imagery, visual and kinesthetic, in order to carry on the movement. There were also those who needed only the instruction verbally and at times not even that." But no one, I take it, believes that the actual neural mechanism of two persons of like age and general motor efficiency is as different as the empirical difference in imagery would imply if the latter be at all really determinant in the performance. This seems to be the general attitude of these 'conclusions' as further inspection thereof would show.

Why, then, does Langfeld say (in conclusion second) "It would be absurd to suppose that the negative [attitude], not to touch the sides, could produce the movement down the board"? In a literal sense, too literal to be more than a quibble, this may be true, but the general consent of the performer to make the experiment at all is what 'produces the movement down the board,' and, actually making it, the intention 'not to touch the sides meanwhile,' is just precisely what does, as a neural mechanism, guide the movement, since to carry out this intention as long as possible is the entire purpose or volition of service as subject. Were not this intention, be it imagery (conscious) or subconscious, the guiding part of the employed neurokinesis, somehow, of course the stylus would most always go anywhere but 'down the board.'

All this seems puerile, almost, in the saying, and it would be so

indeed were it not still the rule for psychologists to strangely enough ignore the inherent inhibitory phase of our motor ideas and with it the whole *inhibitory kinesthetic nature of the great cortex.* But when this is taken into effective account (as it surely will be universally before long) the ingenious discussion about ideomotor action will join the multitude in the limbo of outgrown ideas. For it is *not* the imagery that determines the actual behavior whether the imagery be terminal, that is visual, or kinesthetic, that is current, save *at first*, when that particular coördination of that particular action-system was early in life perhaps being acquired by the psychomotor grey of cord and cortex. One man sees the javelin's mark, the next man perceives the inherent kinesthesia—both meanwhile must keep their consciousness somewhere!, while the ingrained and long-habitual mechanism of receptors and afferent strains and adjustors and efferent strains and effectors hurls the wearied and worn javelin more or less near its goal.

None the less, it is interesting, to me at least, to observe that of Langfeld's five subjects that one ('D') who alone had consistent kinesthetic imagery made the best record, a fact Langfeld fails to note in his conclusions! although obvious in the protocols. His average for the positive instruction with the right hand was 11.2 and with the left 12.9, and for the negative instruction 10.3 with the right hand and 13.2 with the left, as compared with the other four subjects' respective 7.1, 7.0, 7.3, 8.1 and 5.4, 7.7, 11.0, 10.9, 7.2, 8.8, 8.7, 10.1 and 8.5, 8.8, 10.6, 11.8. The figures, then, suggest that, ideomotor action or none, the only subject here who had conscious kinesthetic current control was more skilled at his motor job than the others. Whether cause or effect we need not surmise (unless we presume that it was both), but no one would believe that it was 'chance.'

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THE PSYCHOLOGICAL REVIEW

A SCHEMA OF METHOD[1]

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This paper owes its origin to a study of the historical development of psychological methods to which the writer recently turned his attention. In the course of the investigation, it was soon evident that historically, as well as latterly, the term *method* had assumed a variety of connotations in the literature of the science. An attempt was made, therefore, to determine exactly what these connotations were and to group them under the proper rubrics. The problem, then, changed from a survey of method in general to a classification of the varieties of method. Accordingly a study of the interpretation of psychological methods as outlined in more than a score of treatises was begun. The present article is the result. Its purpose is not to offer an exhaustive logical critique of the concept,[2] but to specify the several usages of the term which were actually found in the literature. After this systematic study has been made, obviously a more adequate review of the historical development of *method* in its several aspects can then be begun.[3]

[1] The article presents, in a revised form and with greater regard for detail, the essential points advanced in the introductory section of a paper which was read before the American Psychological Association, December 30, 1913.

[2] For discussions of this order, the reader is referred to the standard works on methodology and logic, *e. g.*, Jevons, W. S., 'Principles of Science,' 3d ed., London, 1877; Pearson, K., 'Grammar of Science,' 3d ed., London, 1911; Sigwart, C., 'Logic,' 2d ed. (trans. Dendy), London, 1895; Wundt, W., 'Logik,' Vol. 3, 3d ed., Stuttgart, 1908; and 'Encyclopedia of the Philosophical Sciences' (ed. Windelband and Ruge), Vol. 1, 'Logic,' (trans. Meyer), London, 1913.

[3] It is the intention of the writer to publish in the near future a study of this development in terms of the present classification.

If we examine such phrases as *introspective method*, *statistical method*, *adjustment method*, *method of minimal changes*, *inductive method*, *genetic method*, it is clear that the word *method* is not used unequivocally.[1] We find, nevertheless, that in many text-books and systematic treatises these phrases are used coördinately under discussions of *method*. The same thing happens, moreover, in other disciplines. In a recent discussion of the methods of the physical sciences, *mathematical method*, *dynamic method*, *method of thermodynamics*, and *method of analogy*, are treated on a common plane: in a logical outline of the article, faithfully abstracted, these terms would appear as coördinate rubrics.[2] In a standard work on education there are several similar occurrences: *Scientific method*, *schoolroom method*, *psychological method*, and *sociological method* appear on equal terms in the same sentence.[3]

In addition to the fact that the term *method* is frequently used in more than one strict sense, several words are often used in the same contexts with differences of meaning which are difficult to analyse. Examples of the use of the terms *method*, *procedure*, and *instrument*, in this way, are:[4]

> The methods of psychology are, in general, the two methods of every science: description (that is, analysis and classification) and explanation. But besides these

[1] Similar examples can be found in such works as Baldwin's 'Handbook of Psychology,' Vol. 1, New York, 1890, 22ff.; James's 'Principles of Psychology,' Vol. 1, New York, 1890, 185ff.; Külpe's 'Outlines of Psychology' (trans. Titchener), London and New York, 1895, 8ff.; Ladd's 'Elements of Physiological Psychology,' New York, 1887, 6ff.; and in an address on 'Psychological Methods' by W. McDougall, published in 'Lectures on the Method of Science,' Oxford, 1906, 113ff.

[2] Webster, A. G., 'The Methods of the Physical Sciences. To What Are They Applicable?' *Science*, N.S., 39, 1914, 42–51.

[3] Monroe, P., 'A Text-book in the History of Education,' New York, 1911, 757.

[4] In many instances we find the juxtaposition of *method* and *procedure* with meanings which are respectively supplementary, indicating a recognizable distinction:

> In its effort to establish itself upon a scientific basis physiological psychology has no choice but to follow essentially the same method of procedure. Ladd, G. T., 'Elements of Physiological Psychology,' New York, 1887, 6.
>
> The nature of the investigation and method of procedure [section heading]. Swift, E. J., in 'Studies in Philosophy and Psychology,' Garman Memorial Volume, Boston and New York, 1906, 297.
>
> In every case, my plan has been to sketch the development of the test, to prescribe a standard form of apparatus and method of procedure, to explain the treatment of the data secured, and to set forth the results and conclusions thus far obtained. Whipple, G. M., 'Manual of Mental and Physical Tests,' Baltimore, 1910, vii.

fundamental forms of procedure, every science has certain methods peculiar to itself; and the method which distinguishes psychology is that of introspection.[1]

The blunder of the critics of 'introspection' lies in assuming that the results gained in a particular procedure for a particular purpose could be supposed to represent the whole state of affairs. . . . Thus on the question of the validity of introspection we have granted to both parties the main contention—namely, that it is a valid method, and that it is in a degree erroneous.[2]

In psychology the modern transformation comes most strongly out. Here we find an actual department of knowledge handed over to a new class of men for treatment, so remarkable is the demand for scientific method. It is no longer sufficient that a psychologist should be familiar with philosophy and its history, or capable of acute logical criticism of systems; it is necessary, if he would deal successfully with the new problems and gain the ear of the advanced philosophical public, that he should reason from a basis of fact and by an inductive procedure.[3]

One of the methods consists in remarking the disturbances in mental life which occur under conditions of disease and which throw into relief one or another property of our mental equipment. Such a procedure brings to notice the difference between sensation and intellect, the distinction of emotion from either, and the differentiation of volition from all three.[4]

It was to be expected, then, that there would be a longing for some mode of investigation wider in its application and more fruitful than self-observation, and that in due time there would be an organized revolt in favor of 'objective' methods, among which the experimental procedure was to have an important place.[5]

But introspection is not only a method of psychological investigation, it is also a mental fact, and as such it must be capable of psychological analysis and investigation. . . . The second problem is: how far into the nature of the various other mental processes can such an instrument be expected to penetrate.[6]

Nor is this difficulty of analysis confined characteristically to our own language. *Methode*, *Verfahren*, and *Hilfsmittel* occur in contexts in which it is not easy to find logical warrant for a distinctive terminology:[7]

These points of view have to be reckoned with in a discussion of *self-observation* as a psychological method [Methode]. Volkelt forthwith admits that when the intention to carry out the self-observation is presupposed in any individual case, self-observation becomes impossible. If one says on every occasion: Now I will observe, then this purpose will be thwarted at once. He seems to believe, however, that such a procedure [Verfahren] was hardly ever intended.[8]

[1] Calkins, M. W., 'A First Book in Psychology,' New York, 1911, 6.

[2] Scripture, E. W., 'The New Psychology,' New York, 1905, 12.

[3] Baldwin, J. M., 'Psychology Past and Present, Psychol. Rev., 1, 1894, 373.

[4] Angell, J. R., 'Chapters from Modern Psychology,' New York, 1913, 8f.

[5] Stratton, J. M., 'Experimental Psychology,' New York, 1903, 4.

[6] Dodge, R., 'The Theory and Limitations of Introspection, *Am. J. of Psychol.*, 23, 1912, 218.

[7] *V.* also Wundt, W., *Grundriss d. Psychologie*, Leipzig, 1901, 28, and Ebbinghaus, H., *Grundzüge d. Psychologie*, Leipzig, 1903, 578, for similar uses of *Hilfsmittel*.

[8] Wundt, W., 'Selbstbeobachtung und innere Wahrnehmung,' *Philos. Stud.*, 4, 1888, 296.

With this we can close our description of those factors which in general characterize experimental psychology and, according to our examination, distinguish it. We have yet to consider the *method* [*Methode*] to whose application experimental psychology owes its name. Experimentation is not a new source of psychological knowledge for psychologists, but, as has been emphasized often enough on all sides, it is an important aid [Hilfsmittel] in the exhaustive exploitation of sources which are already available.[1]

There is evidence, then, on the one hand, that under *method* we may expect to find a variety of meanings, and, on the other, that in the usage of several different words we are able to trace a common, though loosely defined, reference. In the one case, *method* carries several distinct interpretations; in the other case, various words are used as equivalents for a single meaning. The empirical study of a large number of systematic treatises in psychology reveals, first of all, at least four more or less distinct interpretations in the use of *method;* but it also shows that a trace of these different interpretations is likewise to be found in the several words which are used as synonyms for *method.*

The most frequent meaning of the term is: (1) a general mode of investigation. Examples of this interpretation are: *introspective method*, *method of observation*, *behavior method*, *experimental method.* This group includes all of those more important and typical methods which orientate the science in its investigation and which characterize the general attitude taken in the envisagement of phenomena. These methods are typical, furthermore, of the science considered as a whole, *i. e.*, pertaining to the entire field of psychology. If, any one of these methods, once admitted, were to be discarded, a total change in the point of view, or at least a very radical limitation in the kind of phenomena which are considered psychological, would result.

The name by which the self-ward looking of the psychologist is designated is introspection. In so far as it tends sharply to contrast physical with psychological methods of observation this special term is unfortunate, for as a matter of fact there are no fundamental differences in the methods of the two sciences.[2]

They are methods [experimentation, introspection, etc.], in short, of elevating us above what is purely contingent and accidental in self-consciousness, and revealing to

[1] Külpe, O., 'Anfänge und Aussichten der experimentellen Psychologie,' *Archiv f. Philos.*, 6, 1893, 454.

[2] Yerkes, R. M., 'Introduction to Psychology,' New York, 1911, 41.

us what in it is permanent and essential; what, therefore, is the subject-matter of psychology.[1]

A meaning of *method* which is subsumed under the first rubric and which is, in a sense, a subclass under it, is: (2) a specific type or order of procedure for purposes of control or treatment. Instances of this class are: *method of impression*, *method of average error*, *time-limit method.* The term as here used usually denotes a refinement of experimental method, or of some other method of the first class, and means the manner of handling the material in the control of the investigation. Its application is circumscribed by a particular subject under observation, or by a limited range of attack. It is not applicable to the whole science as such, does not modify the main point of view of the discipline, and can, therefore, be omitted from the list of methods without serious handicap to the science. This type of method, in conjunction with a large number of similar methods, nevertheless, contributes data to psychology.

In the case of the method of minimal change the tests above and below the standard, both working toward it and away from it, must be so alternated as to bring each stage of the test into as nearly like conditions of attention and fatigue as possible; at least when accuracy is an object.[2]

Now, in theory, rate of speed might be measured either by the amount of work performed within a given time or by the time taken to perform a given amount of work, in other words, by a *time-limit* method or by a *work-limit* method.[3]

The word *method* is used in still another sense: (3) the point of view taken, or the intention assumed, in an investigation. This is shown in such designations as: *genetic method*, *comparative method*, *indirect method.* Methods of this class indicate the purpose which prompts the investigation, the attitude assumed in the scientific study. *Genetic methods* would therefore indicate an attitude toward conscious phenomena which has to do with their development and history; these methods serve to reveal facts concerning the genesis of mind. So comparative methods involve a way of viewing mind in the light of its development and of its manifestation in other than

[1] Dewey, J., 'Psychology,' 3d ed., New York, 1891, 13.
[2] Sanford, E. C., 'Course in Experimental Psychology,' Boston, 1908, 348.
[3] Whipple, G. M., 'Manual of Mental and Physical Tests,' Baltimore, 1910, 6.

human organisms. Indirect methods carry the premise that the attack is not inherent in the discipline under whose auspices the method is carried out. In some respects this class of methods resembles the first group; but there are marked differences. The first class has its warrant in the kind of approach; the third class specifies the purpose or attitude behind the approach. The experimental method, for example, when used for the purpose of describing the development of mind in the child, becomes genetic in character; when it is used as a means for timing the reflex arc it becomes indirect; when it is used for the purpose of timing the reflex arc in the child and in the adult for the purpose of deducing genetic relationships, it is both indirect and genetic. Naturally, in a way, experimentation, introspection, and the observation of behavior involve a definite point of view at large, but the words used as adjectival modifiers do not specify the purpose or attitude; nominally the method does not bear the identification necessary to the classification.

This historical unfolding of the course of psychical development is most properly described as the genetic method, that is to say, the method which exhibits the genesis or becoming of things.[1]

The *comparative method*, finally, supplements the introspective and experimental methods. This method presupposes a normal psychology of introspection to be established in its main features. But where the origin of these features, or their dependence upon one another, is in question, it is of utmost importance to trace the phenomenon considered through all its possible variations of type and combination.[2]

The last type of method is sharply demarcated from the others in that, while the latter were classified in terms of empirical differences in the scientific operations involved, it is based on the logical interpretation of these operations. *Method* in this sense is used as: (4) the type of reasoning involved in any of the three preceding forms of operation, or in the systematization of the results obtained, as in: *inductive method*, *method of deduction*, *synthetic method*. As in several other classes, methods here enumerated are not peculiar to psychology, but are characteristic of other sciences as well. Logical analysis of the forms of thought-processes employed

[1] Sully, J., 'The Human Mind,' Vol. 1, New York, 1892, 28.
[2] James, W., 'Principles of Psychology,' Vol. 1, New York, 1890, 194.

in the application of any or all of the other three types of method furnishes the distinguishing principle upon which the fourth class is founded. Such methods,—of generalization, of classification, and of systematization,—are also conspicuously used in the erection of systems of psychology and of the general laws which follow from the operation of other methods.

> To observation, direct and indirect, and to analysis by introspection, reflection, and experiment, we add *induction*—as the necessary method of psychological science. . . . Here, of course, the so-called inductive method implies—strictly speaking—deduction as well as induction, and both analysis and synthesis, after the fashion of the science-making mind of man.[1]
>
> Of the two great historic methods, deduction and induction, recent investigation has shown that neither alone is exclusively productive of great results in the way of discovery or construction, though in their general characteristics and predominant importance, induction may be said to be the method of discovery and deduction the method of construction.[2]

We are now prepared to undertake a comparison of all of these four classes of method in terms of a schema. It is clear that the term *method* has at least four usages. On the principle that it is always bad practice in the field of science to use an expression multivocally, the following proposals are made in regard to the adoption of substitutive phrases for the several meanings of *method:* (1) that *method* be reserved for the first class, *e. g.*, method of experimentation; (2) that *procedure* be used for the second class, *e. g.*, procedure of average error; (3) that *point of view* be substituted for the third class, *e. g.*, comparative point of view; and that *rational principle* be substituted for the fourth class, *e. g.*, rational principle of generalization.[3]

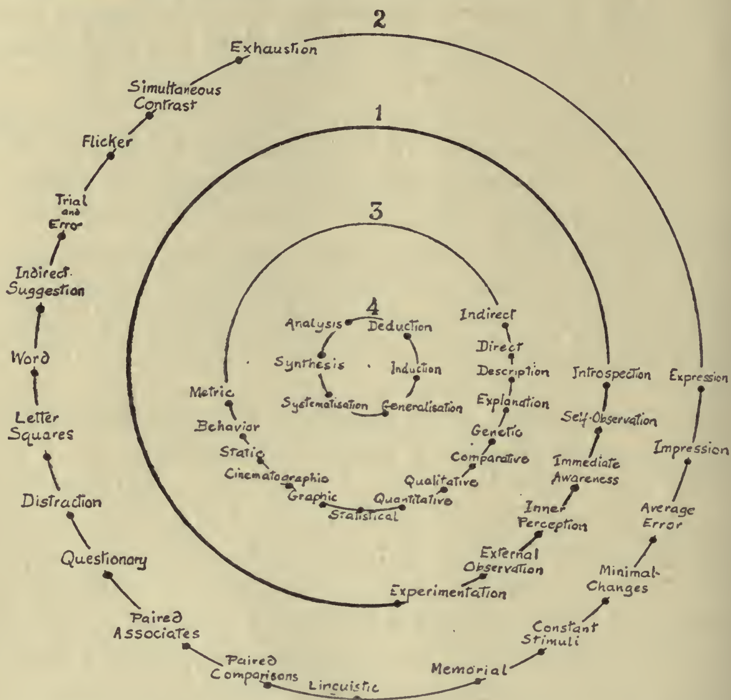
The entire schema then becomes a hierarchy which may be best represented by a fourfold system of orbits, on the pat-

[1] Ladd, G. T., 'Psychology, Descriptive and Explanatory,' New York, 1895, 24–5.

[2] Baldwin, J. M., 'Handbook of Psychology,' Vol. 1, 2d ed., New York, 1890, 20.

[3] Any proposal to modify a practice upon which tradition heavily rests will naturally meet with some resistance through the inertia of the usage: language is not usually so radically reformed or revised as spelling seems to be. But it is hoped that a statement of the question at issue will raise a sane discussion of the merits of the case. In this event, though agreement be not reached, a step will have been taken toward awakening in the writers of text-books and systematic treatises a realization of the necessity for accurate expression in matters of terminology. At any rate, a more pronounced self-criticism in this regard is not too ambitious a standard to erect.

tern of circular slide-rules, but with the added possibility of changing the relative positions of terms in the same orbit. Such a schema is exhibited in the diagram:



Numbers indicate the order in which these classes are discussed. Orbit 1 is heavily lined to show that it represents the class of true *methods*.

On the inner orbit the logical 'methods' or *rational principles* are located, radiating their influence away from the center through all of the other methods. Then come the *points of view*, in turn affecting the operations which lie beyond, but capable of altering their positions so that one *point of view* may be brought in radial position with any *method* or *procedure*. So, likewise, the *methods* may shift positions in the third orbit, bringing any one of the *procedures* in the fourth orbit into radial proximity. As the diagram stands, for instance, the procedure of expression may be used under the introspective method, with a descriptive point of view, and according to the rational principle of induction. The classi-

fication is necessarily incomplete because with the inclusion of other psychological treatises new operations will have to be added. By far the greatest number of operations occur in the orbit of procedures. From within to without a greater refinement is to be noted from the general to the particular.

In conclusion and summary, then, an empirical survey of the literature reveals four classes of usage of the term *method:* (1) as a general mode of investigation, (2) as a specific type or order of procedure for purposes of control or treatment, (3) as a point of view taken, or the intention assumed in an investigation, and (4) as the form of reasoning involved in the pursuit of any of the preceding types of operation, or in the systematization of the results obtained. For these four types of operation it is suggested that the terms, (1) *method*, (2) *procedure*, (3) *point of view*, and (4) *rational principle* be substituted. These classes may be best represented in an orbital diagram or schema in which they assume positions ranging from *rational principles* in the center through *points of view* and *methods*, to *procedures* on the outside.

FATIGUE IN A COMPLEX FUNCTION

BY EDWARD L. THORNDIKE

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By the coöperation of eighty-nine students in a graduate course in educational psychology at Teachers College I am able to report measurements of the effect of about four hours of continuous work at writing poetry upon the quantity and quality of the product produced per unit of time and upon the satisfyingness of the process of producing it.

The work consisted of writing lines to complete 108 couplets, the first lines being given. These first lines were taken from Pope and Byron, the following being a random sampling.

Glittering with ice here hoary hills are seen

____

The fourth day rolled along and with the night

____

Self-love forsook the path it first pursued

____

What she has done no tears can wash away,

____

Bid harbors open, public ways extend

____

From the damp earth impervious vapors rise

____

Mark first that youth who takes the foremost place

____

Back to my native moderation slide

____

But still he only saw, and did not share,

____

In clouded majesty here dullness shone;

____

But while he shuns the grosser joys of sense,

____

But high above, more solid learning shone

____

They were arranged in 9 sets of 12 lines each (called hereafter sets *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h* and *i*). Eight sets were done

without rest in the afternoon or evening of a given day; the ninth set being done after rest in the morning or afternoon of the following day. The individuals who engaged in the experiment were divided into nine squads (called hereafter squads *abc*, *bcd*, *cde*, *def*, *efg*, *fgh*, *ghi*, *hia* and *iab*). One squad did the sets in the order *a*, *b*, *c*, *d*, *e*, *f*, *g*, *h*, rest, *i*; the next squad did the sets in the order *b*, *c*, *d*, *e*, *f*, *g*, *h*, *i*, rest, *a*; and so on. I shall use Period 1, Period 2, Period 3, etc., to designate the nine periods of work. The arrangement of the work was then as in Table I.

Each individual recorded the time required to get the twelve couplets of each set, and also the degree of satisfyingness of the work on each set. The scale for satisfyingness was arbitrarily defined as follows: Call 5 the amount of enjoyment or satisfyingness of mental work which represents your average condition; let 10 represent the greatest amount

TABLE I

THE SET OF COUPLETS COMPLETED BY EACH SQUAD IN EACH PERIOD

Squad	Period 1	2	3	4	5	6	7	8	9
abc	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*
bcd	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*a*
cde	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*a*	*b*
def	*d*	*e*	*f*	*g*	*h*	*i*	*a*	*b*	*c*
efg	*e*	*f*	*g*	*h*	*i*	*a*	*b*	*c*	*d*
fgh	*f*	*g*	*h*	*i*	*a*	*b*	*c*	*d*	*e*
ghi	*g*	*h*	*i*	*a*	*b*	*c*	*d*	*e*	*f*
hia	*h*	*i*	*a*	*b*	*c*	*d*	*e*	*f*	*g*
iab	*i*	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*

of enjoyment or satisfyingness which you have experienced from mental work; let 0 represent the greatest distaste or intolerance toward any piece of work which you have experienced. Let 6, 7, 8, 9 and 1, 2, 3, 4 represent intermediate conditions by equal steps. This scale is obviously crude and unduly subjective, but will suffice for such inferences as will be drawn here.

The quality of the poetry written was measured as follows: The various completions of one couplet (call it *a* 1) were collated and graded by four judges. The same four judges

would grade similarly the completions of another couplet (call it *a* 2). A new combination of four judges would grade the completions of *a* 3 and *a* 4; a new combination would grade the completions of *a* 5 and *a* 6. In all about 80 judges shared in the work of grading the couplets for quality, each judge grading some hundred or more lines; and every line being graded by four judges. The grading was on a scale of 0 to 10, 0 being a line absolutely devoid of merit and 10 as good a line as, in the opinion of the judge, could be written to make that couplet. This again is obviously a very crude and unduly subjective scale, but will serve the purpose of the present argument sufficiently well.

We have then for each of eighty-nine individuals a record like the following of the time required for each of eight consecutively accomplished sets of 12 lines of poetry, and of one done the next day after rest, of the satisfyingness of the work at each of the nine periods, and of the quality of the product produced.

Individual M. D. F. did set *f* in period 1, 7.46.30 to 8.5.45 P.M., spending 25 minutes 15 seconds, the successive couplet-completions being rated as 16, 17, 21, 15, 14, 17, 17, 9, 6, 13, 13 in quality by the sum of four judges' ratings. M. D. F. rated the satisfyingness of the work as 4.

He did set *g* in period 2, 8.8.45 to 8.29.30 P.M., spending 20 minutes 55 seconds, the successive couplet-completions being rated as 14, 19, 20, 11, 14, 16, 20, 15, 12, 7, 18, 13 in quality by the sum of four judges' ratings. M. D. F. rated the satisfyingness of the work as 4.

And so on for the seven remaining sets.

These individual records are arranged in 9 squads so that each set (*a*, *b*, *c*, *d*, etc.) was done at each period, equalizing the effect of any differences in the difficulty of the sets of first lines to be made into couplets.

Examination of the individual records shows that for the problem under consideration there is no misleading in massing the results and presenting simply average or median achievements and degrees of satisfyingness by squads. This is done for time required, quality of product, and reported

satisfyingness in Tables II., III. and IV., which utilize all the records for Period 1, Period 8 (the last of the consecutive work-periods), and Period 9 (the work-period of the following day after rest).[1]

The facts are clear. The speed of work increases throughout the work-period and is not benefited by the rest. The average quality of the product produced falls off a very little, from 4.47 in Period 1 to 4.24 in Period 8, and is slightly benefited by the rest, from 4.24 in Period 8 back to 4.47 in Period 9. The reported satisfyingness of the work falls off greatly, from 5.38 in Period 1 to 3.56 in Period 8, and is greatly benefited (from 3.56 to 4.85) by the rest. The effect of continuous exercises of the function is to increase gross efficiency, but to decrease satisfyingness or interest. The effect of the rest is a very slight gain in gross efficiency but a very great gain in satisfyingness or interest.

Table II

Times Required to Complete Sets of 12 Couplets at the Beginning of the Work-Period (Period 1), at the End of the Work-Period (Period 8), and on the Next Day After Rest (Period 9)

Squad	Period 1	Period 8	Period 9
abc	29.4	19.3	21.7
bcd	37.4	20.7	21.9
cde	39.3	22.6	22.4
def	35.1	23.3	22.6
efg	44.5	27.0	29.7
fgh	36.4	24.6	23.7
ghi	27.7	22.8	25.3
hia	30.4	28.3	23.5
iab	30.0	18.3	17.8
Average	34.5	23.0	23.2

[1] Some of the eighty-nine individuals did not complete all of the 108 couplets within the approximate four hours set apart for the experiment. Some others by accident failed to complete the entire set of couplets or to score the time.

In all there were fourteen such individuals out of eighty-nine making 27 such blanks out of 801. Where an individual did not do all eight sets in the work period the last set that he did do was treated as the set to be counted under Period 8 in Tables II., III. and IV.

Table III

Median Quality of Poetry Written at the Beginning of the Work-Period (Period 1), at the End of the Work-Period (Period 8), and on the Next Day After Rest (Period 9). The Quality Measure is the Sum of Ratings by Four Judges

Squad	Period 1	Period 8	Period 9
abc	15.5	13.0	14.5
bcd	19.0	17.0	19.0
cde	18.5	17.0	18.0
def	17.0	18.5	20.0
efg	18.5	17.0	17.0
fgh	18.5	16.5	17.5
ghi	20.0	19.5	19.5
hia	17.5	17.0	18.0
iab	16.5	17.0	17.5
Average quality	4.47	4.24	4.47

Table IV

Reported Average Satisfyingness of Writing Poetry at the Beginning of the Work-Period (Period 1), at the End of the Work-Period (Period 8), and on the Next Day After Rest (Period 9)

Squad	Period 1	Period 8	Period 9
abc	4.75	3.42	4.42
bcd	5.55	4.89	5.67
cde	5.30	3.40	4.90
def	4.20	3.70	4.70
efg	5.09	2.64	3.91
fgh	5.75	3.52	4.86
ghi	6.50	3.88	5.38
hia	5.86	2.71	5.71
iab	5.43	3.86	4.14
Average	5.38	3.56	4.85

The Curve of Work

The average amount per unit of time and satisfyingness reported for each successive period of the total work period and the quality of the product in periods 1, 4, 6 and 8, were as follows:[1]

[1] The results given here are not for equal *time* periods but for successive sets of twelve of the *couplets*. Also there were necessary a few adjustments in the case of the 3½ per cent. of blank records referred to in a previous note. The average times of periods 1, 2, 3, 4, etc., were approximately 35, 28, 26, 25, 25, 24, 24 and 23 minutes.

Period	1	2	3	4	5	6	7	8
Couplets per 10 minutes	3.48	4.36	4.69	4.82	4.84	5.08	5.03	5.22
Quality of couplets	4.47	...	...	4.43	...	4.35	...	4.24
Satisfyingness	5.38	5.21	4.94	4.48	4.31	3.95	3.67	3.56

The changes from period to period were thus:

	1–2	2–3	3–4	4–5	5–6	6–7	7–8
Couplets per 10 minutes	+.88	+.33	+.13	+ .2	+.24	−.05	+.19
Satisfyingness	−.19	−.27	−.46	−.17	−.36	−.28	−.11
Quality of couplets	−.04 from 1 to 4			−.08 from 4 to 6		−.11 from 6 to 8	

These facts are also clear. Speed improves, with fluctuations such as one customarily finds. Quality remains about the same. Satisfyingness falls off throughout.

ON THE READING AND WRITING OF MIRROR-SCRIPT

BY JUNE E. DOWNEY

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§ 1. Report on a Case of Spontaneous Right-Hand Mirror-Writing

During the college year 1912–1913 the writer had under observation a third-year college student, twenty-five years of age, referred to as *X*, who reported some peculiar conditions existing relatively to a long-continued habit of *right-hand* mirror-writing. Study of the case suggested certain significant questions of general interest in connection with the problems of visual and motor orientation. The purpose of this paper is to state these problems and to discuss them briefly in the light of the data at hand.

The facts reported by *X* were obtained from answers in writing to a set of questions submitted by the experimenter. A duplicate set of questions was sent to *X*'s mother in another town and the independently obtained answers compared.

X, it appeared, learned to write at about five years of age by copying the writing or printing of her mother who sat across the table from her. After watching the mother's movements across the table, *X* would be given the copy to imitate. *X* wrote mirror-fashion in these early attempts and during the whole of her first year in school, during most of the second year, and a little during her third year. The teacher who succeeded in breaking *X* of the habit exercised great care, requiring written work to be done over and over until it resembled normal productions. Not until about ten years of age was the child thoroughly broken. *X* adds that even at the present time, when very tired, she reverts to mirror-writing.

X reported that she had a strong tendency to read from

right to left instead of from left to right and that this habit persisted about as long as the mirror-writing habit, adding, "I don't believe I am really broken of it as I will sometimes read three or four lines that way before I think what I am doing." There was a tendency at first to pronounce words backwards, "then as I grew older and could recognize words such reading affected merely the sequence of words." During the time she was under observation, *X* complained of difficulty in mastering first-year French, due in part to attempts to pronounce the words according to the sequence of letters from right to left. *X*'s mother reported in addition to these facts that in *X*'s first attempts at number-work the answers were placed on the top of the examples. Also, that *X* had, as an infant, crawled backward.

X's further observations were very significant. "I worked in my cousin's newspaper office for about a year. It was no trouble at all for me to learn to set type. Within a month I could set type as fast as my cousin could, although he had worked at this ten or more years."

"When handwriting is not familiar to me I have to spell most of the words out. After a month or so of teaching I could read the writing of my pupils readily but was never able to tell which pages belonged to which child when no name was signed. Such a condition held even after two years' work with certain children. I would always have to spell out names that were not written very plainly—every letter distinctly." *X* reports, further, that she cannot recognize differences in the writing of her father, mother, and an older sister.

X was unable to pass an examination for post-office work which consisted in reading a given number of addresses in a given time. She served, however, as a postmistress for a year before teaching and was obliged to give up this work because of the great nervousness ensuing, due to the strain involved in the reading of many different hands.

The contrast in the ease with which *X* took up typesetting and the difficulty she found in post-office work and in general in the reading of script is instructive since it points

to the greater facility with which she makes shifts in position in comparison with shifts in form.

Again, X reported that in memorizing a page it is placed preferably to the right; the page is then memorized in visual form but turned over and in front, that is, in the imaginal representation, the page is seen in mirror-fashion. To copy this, the page is again turned over in imagination but now towards the left. In reciting orally, X reads from the front page where the type is seen in reversed form. In order to memorize a selection to the best advantage, X copies it on a blank sheet of paper using one side only as there is a conflict if both sides are written upon. If both sides are written upon the first sheet is more easily visualized than the second.

The points of interest in this general report are (1) the association of spontaneous right-hand mirror-script, induced perhaps by the method of learning to write, with general confusion in the matter of motor orientation; (2) the ease with which X, according to her report, is able to handle shifts in position of letters in comparison with shifts in their form; (3) the suggestion that the mirror-reversal may also affect imaginal representations.

Before formulating questions relative to the general significance of such reported facts the writer made an attempt to test these assertions of X and, also, to determine by means of control tests on other reagents the extent to which X showed unusual capacity in dealing with displacements with subcapacity in dealing with differences in form.

In class work with X the writer noticed some peculiar conditions, for instance, a quick verbal memory with considerable incapacity in dealing with meanings, a point of some interest as will be seen in a later connection. Shifts in terminology always confused X greatly. Furthermore, although timed tests upon rapidity of writing showed that X, in comparison with other students, is a rapid and excellent penman, she composes with very great slowness, in fact so slowly as to indicate some inhibiting tendency at work. It will be recalled that X reported that when fatigued she has a tendency to revert to mirror-writing. She was also of the

opinion that she would be better able to compose if allowed to write in mirror-script.

The following test was tried. *X* was given two questions in a class quiz. The first question she was instructed to answer in normal script, the second, in mirror-script. These answers were completed in twenty minutes. Then, unanticipated by *X*, the instructions were reversed: the first question was to be answered in mirror-script; the second in normal script. Again, twenty minutes were consumed. The results did not seem to confirm *X*'s opinion. In both cases the questions were answered in mirror-script less fluently than in the normal. 140 words were written in normal script in answer to the first question; 120 words in mirror-script. In the case of the second question, 52 words were written in mirror-script, 115 words in the normal. There was no evidence of greater ease of thought in writing the mirror-script. The second twenty minutes produced 235 words as over against 192 words in first twenty, but with considerable repetition of thought and reversion to the earlier wording. The test served, however, to show definitely the slowness with which composition proceeds and points to a retardation of some kind.

Memory tests on *X* were not carried to completion. Many times, however, in the course of these as well as in tests on writing under distraction *X* reported a visual verbal image which was seen in reversed form. In general, it may be noted that *X* makes much use of visual imagery. Specifically, in the memory tests, this reversed image appeared largely when *X* sought to memorize a passage by first *writing* it out and then memorizing it visually.

A test on the memorizing of two passages in prose, which *X* first wrote on a sheet of paper, gave some interesting results, which, however, need confirmation by further work. Briefly, the following points were noticed. The first passage, one of eight lines, was written by *X* upon a blank sheet of paper. When *X* thought that she knew the passage she wrote it again and compared her reproduction with the original passage, concentrating upon the more difficult parts. When confident of the whole she repeated it to the experi-

menter, reading from a visualized copy of the passage as it appeared the time of her second writing, the whole being seen on a level with her eyes, from the other side of the paper, that is, in mirror-script. Ten and a half minutes were taken for memorizing the passage; four trifling errors were made in the reproduction.

The second passage, of eight lines taken from the same book as the first, was memorized as before except that it was written upon a sheet which contained writing on the reverse side. The memorizing of this passage required eleven and a half minutes, with eight errors in reproduction, much more serious errors than occurred before. The content may, of course, have been responsible for the increased difficulty. The passage was read off in mirror-script as in the preceding test but certain words were blurred. *X* reported that, so far as she was able to judge, the writing on the other side of the paper had nothing to do with this blurring. The memory was at fault. The experimenter noticed that *X* turned greatly to the left in attempting to recall the passage. This turning to the left appeared also in a second reproduction of the first passage. When instructed to turn to the right and repeat the second passage, *X* hesitated for fifty seconds and then said, "Picture won't come!" Directed to turn toward the left, after thirty seconds' hesitation, she was able to see certain words and to give correctly the last line as a whole. Turning towards the right caused a blurring of this line. A third repetition of the first passage when *X* was turned first towards the right, then towards the left showed greater control of the visual imagery in the second case with the giving of words that had been blurred during the preceding visualization.

These observations are included in the report with some hesitation since the writer is conscious of their inadequacy. They suggest, however, interesting lines of investigation.

X's capacity in the reading and writing of mirror-script was determined by tests in which her skill was compared with that of eight other third-year college students. Later, as will be described in another section of the paper, these

tests were made much more extensive but with little additional information so far as *X*'s comparative skill was concerned.

The first test tried was on the rapidity with which a reagent could read a sentence written in large formal mirror-script. This test will be described in detail further on in the paper. At this point in the discussion the following observation alone is important, namely, that among the nine reagents compared, there are four who read the sentence more quickly than *X*, four who read it more slowly. The exact records of this group are given in Table I., Group II. The range in time is, it should be noticed, very great, extending from 33 seconds to over four minutes. *X*'s time is two minutes and fifteen seconds.

A second much more difficult test on the reading of mirror-script was tried on seven of these reagents, *X* being one of the seven. In this test a postal-card was utilized containing some fifty words written in mirror-script that possessed the striking individuality of usual writing. The time needed to read this card ran from twenty-seven minutes to over an hour. Among these seven reagents *X* takes third place, the two who excel her, and that slightly, being those who occupied the first and third place in the preceding test. On the whole, *X* shows greater comparative efficiency in this test than in the first.

In the third test mirror-type was utilized. Again, *X* was third of six subjects. Throughout these three tests there is very little shifting in relative position. Subject *B*, who showed great proficiency in the ease with which he read mirror-reversals, maintained first place in all these tests. *X* was less able to utilize meaning in her reading than were certain other of the reagents. She also complained of the peculiarity of the writing on the postal-card, and repeated her assertion that she always found difficulty in reading a strange handwriting.

On the whole, it appeared from the tests that while *X* showed a certain amount of efficiency in the reading of mirror-script, there were unpracticed reagents who excelled

her. The test served, however, to emphasize the extraordinary differences found among individuals in their ability to deal with the material under consideration.

In order to test *X*'s assertion that she found great difficulty in reading unfamiliar writing or writing that in any way varied from formal script, records were made of the rapidity with which she and the eight other reagents of the first mirror-reading test were able to read a letter written in a peculiar and somewhat difficult hand. This test was also utilized later in a more extensive way and will be described in some detail in an additional report. There were two parts to this test. In the first part the reading of the first page of the letter, consisting of seventy-one words, was timed. In the second part, the reading of the second page of the letter, seventy-one words as before, was timed. In this second test, however, an attempt was made to get rid of context and, hence, of meaning, by blotting out intermediate and significant words. Meaning was, however, by no means completely obliterated. A comparison of the results from the first and the second test for any particular reagent served to bring to light the relative dependence upon meaning in the first and upon visual habituation in the second case. These individual differences will be discussed in another connection. So much may be said here. The range in time for reading the first page ran from 57 seconds to six minutes. *X*'s time was four minutes fifty seconds, with five errors. Four of the reagents were more rapid; four were slower. In the second test, seven were more rapid than *X* and only one was slower. To a slight extent, then, *X*'s opinion was confirmed. Furthermore, when tested on her ability to identify specimens of handwriting produced by the same penman, *X* proved to be the most inaccurate of the group with one exception.

These tests served to throw into the foreground extraordinary individual differences and suggested a series of tests which the writer has at present under way. The comparative rating of *X* in these tests and in those on mirror-reading cannot, of course, be cited in any conclusive way. The situation suggests, however, a problem of some interest,

namely, the ability to deal with position as distinct from the ability to deal with form.

X's skill in mirror-writing was also determined by control tests upon eight other reagents. These nine subjects all wrote the same familiar verse, first, with the right hand in normal script; second, with the right hand, mirror-script. Their rapidity was timed with a stop-watch. The test was then repeated with the left hand.

Of the nine subjects, there were two who wrote the given verse slightly more rapidly (25 and 14 seconds) than *X* in the right-hand mirror-script; there was one who was more rapid in the left-hand mirror-script. These subjects were not those who had excelled in the reading of mirror-script. In both cases, however, *X* produced copy that was superior to that of the more rapid reagents. *X*'s mirror-script, however, although of fair quality, does not compare in appearance with her normal right-hand writing. Moreover, she produces it much more slowly than the normal as shown by the fact that the given verse was written in the usual manner in 59 seconds, in mirror-script in three minutes eleven seconds. *X*'s normal writing with the left hand showed no superiority either in rapidity or appearance; the mirror-writing, left hand, four minutes three seconds, compared favorably with that of the other reagents.

As a control in both left- and right-hand mirror-writing, *X* reported that she visualized the verse *as a whole* in mirror-script and copied this verse word by word without attempting vocalization of the words. Visualization in mirror-script of the more difficult words appeared also when *X* attempted to write the verse in normal fashion but under various forms of distraction. *X* was able to maintain mirror-writing both while counting aloud and while reading aloud with no more effort than was found necessary in maintaining normal writing under such conditions.

As a net result from the tests we may conclude that, in spite of her habituation to mirror-forms, *X* shows no striking superiority to certain unpracticed reagents. Such a conclusion raises a number of general questions: (1) the extent

to which individuals differ in their capacity to interpret mirror-reversals; (2) explanation of such individual variation; (3) the extent to which ability in reading mirror-script (visual) is correlated with ability to write it (motor) and with ability to read and to write inverted script; (4) the relative skill of the right and the left hand in the production of mirror-script; (5) the relation of capacity in mirror-reading with capacity to interpret form in general. Before citing tests that attempt to answer the first four questions, it may be well to review certain experimental studies that throw light upon the general problem.

But, first, a word concerning the appearance of mirror-reversals in imaginal form.[1] Janet in an interesting article has suggested that reversal of position in one's visualization of a situation may account for certain illusions of orientation. The writer of this paper has had occasion to note in two or three imagery tests left-right inversions of objects in a visualized whole. The point is emphasized here as one that merits further investigation.

§ 2. Literary Setting of Problem

An increasing interest in the tendency of certain children to write mirror-script instead of normal left-right script is shown by the reports on the subject that have been multiplying within the last ten years. The chief value of these reports has consisted in determining the conditions under which spontaneous mirror-writing appears and in proving that the early conclusion that mirror-writing is the normal writing of the left-handed or of the left hand in general when the right hand becomes incapacitated is an inadequate generalization.

Stern[2] has furnished an illuminating discussion of the subject and succeeded in bringing it in line with the general problems of space perception and motor orientation.

[1] Janet, P., 'Le renversement de l'orientation ou l'allochirie des representations,' *Jour. de psychol., norm. et path.*, 1908, V., 89–97.

[2] Stern, W., 'Ueber verlagerte Raumformen,' *Ztscht. f. ang. Psychol.*, 1909, II., 498–526.

Stern has shown, by his own observations and those of others, that spatial displacements are common occurrences in the early drawings and attempts at writing of the right-handed child as well as of the left-handed one. Furthermore, a point which needs emphasis, the right-left displacement resulting in so-called mirror-writing or mirror-drawing is not the only form of shift in position that occurs. A child in first attempting to make numerals may, for example, turn them completely upside-down or may turn them toward the right 90 degrees so that they appear lying upon the side. Nor is the problem one that concerns motor phenomena exclusively since the child also shows ability in interpreting perceptually forms that are shifted from their normal position. Various observers from Sully on have reported that the young child finds no difficulty in enjoying pictures that are held upsidedown or otherwise turned from their usual position.[1] This apparent indifference of the young child to position can, Stern insists, be understood only in the light of the development of space perception. There exists in the child a perception of form with an apparent indifference to position that seems to indicate that while form is nativistically determined, perception of position is an outgrowth of experience. Aboveness, belowness and the other directions represent not pure optical data but originate through the association of determined optical impressions with certain movements of the body. In the child fusion of form and position has not yet resulted, hence the ease with which he produces and interprets given forms in any position. Even in the adult this connection does not have the stability of inborn sensory connections.

The further question why variations occur in the propensity of children to produce such forms is raised. Partly, Stern insists, these variations depend upon external conditions which may make permanent a form of displacement originating in some accident. Partly, these variations are thought to be dependent upon inner conditions, since the child visually preoccupied will pay less attention to the posi-

[1] A child of two years whom I tested recently recognized without difficulty in an inverted picture faces measuring only one eighth of an inch across.

tion of a given form than will the child of a more motor type who comprehends optical representations egocentrically, that is, with reference to movements executed in their presence whereby their position becomes fixed. Indifference toward spatial position may also be due to failure to assume a practical attitude toward a seen object,—an interesting point in connection with *X*'s failure to reckon with meanings. The more space-forms take on a practical social aspect, the more their position becomes an integral part in the comprehension of the whole. While production of mirror-writing is in itself no sign of deficiency, a continued failure to note its difference from usual writing may indicate weakness of comprehension. Mirror-writing with the left-hand may also result, Stern holds, as a consequence of practice in normal writing with the right hand.

Stern's formulation of the shifting relation between spatial form and spatial position in the course of development is very suggestive. Individual variations even in children relative to the closeness of the association of these two elements arouses the suspicion that in adults also variation may exist as to the degree of stability of the connection, a conclusion in line with the observations reported above.

In this connection the experiments of Paula Meyer[1] upon the memory for simple forms are in point. Meyer gives the percentage of cases in which figures reproduced from memory are reproduced in some form of reversal and finds, in support of Stern, that such reversals of form occur more frequently in the case of children than of adults if reversals of all kinds are considered. So far as the mirror-reversal is concerned specifically, the tables show certain conditions under which mirror-reversal has occurred more frequently in the case of the adults than in the case of the children. Since, however, reagents as well as conditions were shifted in the various series of tests and the reagents in each particular series were few in number, it is quite possible to conjecture that individual variation influenced the outcome to a considerable degree, a point, of course, worth clearing up.

[1] Meyer, P., 'Ueber die Reproduktion eingeprägter Figuren u. ihrer räumlichen Stellungen bei Kindern u. Erwachsenen,' *Ztscht. f. Psychol.*, 1913, 64, 34–91.

In any case, Meyer finds the mirror-reversal a more frequent occurrence than the up-down displacement. Dearborn[1] in testing recognition under objective reversal had found, on the contrary, that "an object is recognized more readily when inverted than in either of the two intermediate positions of quarter-reversal, and more readily than in the erect mirror-position or that position inverted." He found the next most favorable position for recognition to be that of the erect mirror-position. It is quite possible, of course, that the form of displacement most easily recognized may not be that which most frequently occurs in a motor reproduction. The discrepancy, however, deserves attention.

From neither of the above reports is it possible to determine to what extent individual variations occurred since the results are presented in massed form. As suggested above, Meyer's separate tables indicate, it would seem, considerable individual difference in the tendency to produce mirror-forms in the case of both adults and children.

Although both reports give certain individual introspections, it is, again, not possible to determine whether there exists any correlation between imaginal type and frequency of reversal in reproduction or ability to recognize a figure when so reversed.

A general survey of the situation suggests, therefore, the same problems as those raised in the preceding section, particularly with reference to the extent of individual variation in the capacity to interpret and produce spatial displacements.

One recalls at this point another series of experiments, in which so-called mirror-drawing has been utilized in order to test speed in the formation of new associations, or, more specifically, to test adaptability and its correlation with general intelligence (Burt),[2] the process of learning by trial and error and cross-education (Starch),[3] individual and sex

[1] Dearborn, G. V. N., 'Recognition under Objective Reversal,' PSYCHOL. REV., 1899, VI., 395–406.

[2] Burt, C., 'Experimental Tests of General Intelligence,' *Brit. Jour. Psy.*, 1909, 3, 94–177.

[3] Starch, D., 'A Demonstration of the Trial and Error Method of Learning,' PSY. BULL., 1910, 7, 20–23.

differences and practice effects (Whipple).[1] Calfee[2] has also utilized mirror-tracing in a recent test upon college freshmen with the suggestion that the discrepancy between her results and those of Burt may be due to a growth factor, a point of interest in the present connection. While these tests bear some relation to those just cited, there are some striking differences in the general situation, for in drawing or writing in which one perceives the result of the movement in the mirror, the readjustment required is that of an old movement to a new visual report, and success may be achieved by a voluntary ignoring of the visual report, while in production of mirror script the direction of movement is actually reversed.

The mirror-tracing tests also show great individual differences in the ease of adjustment. Whipple gives a time-range for adults in the first attempt at tracing the star from about 50 seconds to over 8 minutes. A sex difference is also suggested, indicating faster speed for girls and women than for men and boys.

The tests to be discussed in the following section of this paper make, of course, no claim to be an adequate treatment of the problems under consideration. They are concerned, chiefly, with certain features of interest in the reading and writing of mirror-script, with an extension to the reading and writing of inverted-script. Furthermore, an attempt is made to answer a few questions by working out certain correlation coefficients according to Spearman's foot-rule method as described by him in the *British Journal of Psychology*.[3] It is obvious, however, from current discussions upon the subject of correlation, that assertions based upon the use of any correlation formula must be more or less open to suspicion until a more general agreement is reached as to the demands of the method. No attempt was made to correct these correlation coefficients by a second test upon the same or

[1] Whipple, G. M., 'Manual of Mental and Physical Tests,' 1910, 343–349.

[2] Calfee, M., 'College Freshmen and Four General Intelligence Tests,' *Jour. Ed. Psy.*, 1913, IV., 223–231.

[3] Spearman, C., '"Footrule" for Measuring Correlation,' *Brit. Jour. Psy.*, 1906, 2, 89–109.

other reagents. A second test on the same reagents would have introduced habituation, which from my experience with reagents I judge to be very rapid in the case of mirror-writing but rather slow in the matter of reading of mirror-script.

It is, however, very possible that the determination of individual capacity in the matter of rapid habituation to either the reading or writing of mirror-script might prove of more value in the rating of individual capacity to deal with shifts of position than a ranking based on a first test. Furthermore, any striking difference in ease of adjustment to the motor test in contrast to the visual one would be very significant. Lack of time has prevented my carrying out such tests on habituation, although the report is incomplete without them.

§ 3. Individual Variation in the Ability to Read Mirror-Script

The reading of mirror-script was utilized in my monograph on control processes in modified handwriting[1] in the hope of throwing into the foreground certain individual methods of procedure. 3 *B*, Part I. of this monograph gives the reaction of nine subjects. Even among these reagents the individual variation in the ability to read a sentence in mirror-script was enormous, ranging from 40 seconds to over 25 minutes.

In order to obtain enough records to make certain comparisons possible seventy more records were obtained from as many different reagents, ranging in age from eight to seventy years. The manner in which these reagents were selected will appear later. Each reagent was tested separately and according to standardized instructions which were typed and placed before the experimenter. Five experimenters besides myself were utilized in giving the tests. All were members of my class in experimental psychology and worked under my personal supervision.

Method.—In order that results might be comparable with

[1] Downey, J. E., 'Control Processes in Modified Handwriting,' Psy. Rev. Mon., 1908, 9, No. 37.

those of the earlier test, duplicates of the card used in the monograph test were used in the present series. The sentence, "Two telephones were placed at two symmetrical points of the same circle," was written in large formal vertical script upon transparent white paper of good quality. This paper was then reversed and pasted upon a white card so that the script appeared in mirror-script. Before the exposure of the card, the reagents (except the children) were told that the time taken by them for the reading of a sentence written in mirror-script was to be obtained. If the reagent volunteered any questions concerning mirror-script, these questions were answered. As the card was turned into reading position by the subject, a stop-watch was clicked by the experimenter. Children were told that they were to read a sentence that ran from left to right (the direction being indicated by a gesture) instead of running in the usual direction of writing. They were told to spell out words if they chose. In every case an attempt was made to hold a reagent to the reading until every word had been correctly given. In a very few cases this proved to be an impossible task. On the basis, therefore, of the timed readings that had been obtained for individual words from a large number of reagents, the relative difficulty of the words was determined and arbitrary values assigned to each word. The rapidity for reading the whole was then calculated on the basis of the number of words read in the given time. These calculated values were, however, utilized in only five instances, and occur only in the case of excessively slow readers.

Results.—These seventy reagents show, as did those of the monograph test, surprising variation in ability. The range in time is from 25 seconds to about 47 minutes. This last record represents, it may be said, a calculated value since the reagent in question succeeded in interpreting only the simple words in the sentence.

There are 12 reagents, among the 79, who read the sentence in less than 60 seconds, with an average for this group of 38.9 seconds. There are 13 records that run over 8 minutes 30 seconds with an average of about seventeen

minutes. Very crudely, we may say, then, that the best group is about 26 times more able than the worst group.

The median value of these 79 measures is 3 minutes, 35 seconds and the approximate P.E., found as suggested by Whipple[1] "by counting off one fourth of the cases from either end of the series of measurements and halving the difference between the two values so found" is 2 minutes 2.5 seconds, that is, 39 cases fall within the values of 1 minute 30 seconds and 5 minutes 37 seconds.

Before considering attempts to relate such individual differences to other factors in the reagent's make-up, it may be well to discuss the possibility of practice effects entering into the results to any considerable extent. Every reagent was questioned as to whether he had, previous to the test, attempted to read mirror-script. Subject X, whose case was reported above, was the only subject who reported extensive practice. She ranks twenty-fifth among the seventy-nine reagents. A few others reported slight practice in the use of rubber stamps or in type-setting and one or two remarked that they had attempted to read, successfully or unsuccessfully as it might be, souvenir postal cards that contained sentences in mirror-script. The reagents who reported such practice made the following ranks: 1, 2, 3, 6, 25, 46, 56, 62. On the whole, an impression was obtained that a tendency to utilize or to amuse oneself with mirror-forms is to some extent indicative of readiness in dealing with them. As was suggested above, tests to determine the limits of practice for a large number of subjects are highly desirable.

In considering individual differences, the following factors seem to merit discussion, first, the influence of age, which as Stern's observations suggest, may determine the extent of fusion of visual and motor spatial factors; secondly, sex, since Burt and Whipple in the mirror-tracing tests find greater speed on the part of women and girls, a superiority which Whipple relates to the greater use of the mirror by women but which Burt is inclined to attribute in part at least to an innate sex-difference; thirdly, the influence of certain perceptual and imaginal predispositions.

[1] Whipple, G. M., 'Manual of Mental and Physical Tests,' p. 18.

I am inclined to believe, further, that family resemblances deserve study in this connection. Striking similarities in reaction would point to innate difference as basal in the explanation of readiness or awkwardness in dealing with spatial reversals. The following facts may be of interest. Four members of the writer's immediate family took part in the above test. One ranked thirty-ninth, the others, sixty-third, seventy-third, and seventy-sixth. Two other members tried after the completion of the test, after staring helplessly for three minutes at the test-card without succeeding in reading even the simplest words, refused to make further effort. One remarked that he had often tried to read mirror-typewriting but always unsuccessfully. Two of the slowest of the subjects, occupying the seventy-fifth and seventy-seventh places, are sisters. The reagents ranking respectively sixty-six and seventy-four are mother and son; those ranking second and twenty-second, brother and sister; those ranking twenty-fifth and sixty-fifth sisters.

Should striking innate individual differences, beyond the influence of practice effects, be found, they might well prove of practical significance as well as of theoretical interest in their bearing upon the problems of spatial orientation. It may be conjectured that the difficulties of certain students in dealing with stenographic symbols may be explained by further investigations along this line. Varying skill in type-setting may also, in part, be so explained. The reagent who ranged eighth in the above test belongs, it may be said, to a family of printers. Her father and her mother's brother are both type-setters of unusual skill.

So much by way of suggestion. To return now to a consideration of the records.

Before proceeding with the discussion of the effect of age upon the speed with which mirror-script is read, a word as to the selection and grouping of the reagents. The first group is composed of the nine reagents of my monograph, graduate students or instructors in psychology. These reagents are here referred to by the Roman numerals I., II., III., etc.; they ranged in age from the middle twenties to

the middle thirties. The second group is composed of nine reagents, all juniors in college. This group includes *X* and the students tested with her as control reagents. The members of this group are referred to by the capital letters; they range in age from nineteen years eight months to twenty-four years eight months. The third group is made up of adults of the writer's acquaintance. These reagents are referred to as A_1, A_2, A_3, etc.; they range in age from the middle thirties to seventy years. A last group is composed of nine children from the fifth and sixth grades, sent to me by the supervisor of the training school. These children are referred to as C_1, C_2, C_3, etc.; in age they range from eight years three months to twelve years two months. The youngest child, who has since been promoted to the sixth grade, is obviously a child of unusual ability. The oldest child, C_9, was so careless and indifferent as to make testing him a difficult undertaking.

After the testing of these groups was completed, the second group was enlarged to include, as a whole, 25 seniors and juniors ranging in age from eighteen years ten months to twenty-six years nine months. Furthermore, 25 freshmen girls were tested. These last reagents are referred to by the small letters of the alphabet; they range in age from seventeen years four months to twenty-four years two months. Two sophomores were tested, bringing the whole number up to seventy-nine.

A. Age and Variation in Ability to Read Mirror-script

Stern's and Meyer's results suggest that the varying ability to read mirror-script may, in part at least, and up to an undetermined age, be dependent upon age. In the attempt to determine the facts as to such a possible correlation, the records from the first four groups of subjects were tabulated (see Table I.).

In general, it may be said, the test was not fair to the children, since the sentence used contained words with which the children were unfamiliar. Moreover, the children were unable to profit by the context as did many of the other

Table I

Rapidity in Reading Mirror-script

(The order within each group is in the order of age, the youngest first. M. and F. indicate the sex of the reagent.)

Group I. (Graduate Students and Instructors in Psychology) Reagent	Time	*Group III.* (Adults, All Over 34 Years of Age) Reagent	Time
I. (F.)	Over 25 mins.	A_1 (M.)	12 mins. 30 secs.
II. (M.)	1 min. 14 secs.	A_2 (M.)	4 mins. 17 secs.
III. (F.)	1 min. 35 secs.	A_3 (F.)	45 secs.
IV. (M.)	9 mins.	A_4 (F.)	5 mins.
V. (F.)	40 secs.	A_5 (F.)	5 mins. 30 secs.
VI. (M.)	9 mins.	A_6 (F.)	8 mins.
VII. (M.)	7 mins. 30 secs.	A_7 (F.)	8 mins. 24 secs.
VIII. (F.)	16 mins. 51 secs.	A_8 (M.)	47 secs.
IX. (M.)	3 mins. 10 secs.	A_9 (M.)	3 mins. 35 secs.
Average	8 mins. 13 secs.	Average	5 mins. 25 secs.
M.V.	5 mins. 59 secs.	M.V.	2 mins. 50 secs.

Group II. (College Juniors) Reagent	Time	*Group IV.* (Grade Children) Reagent	Time
B (M.)	33 secs.	C_1 (M.)	2 mins. 50 secs.
C (F.)	1 min. 10 secs.	C_2 (M.)	2 mins. 58 secs.
D (M.)	37.6 secs.	C_3 (M.)	4 mins. 40 secs.
E (F.)	3 mins. 30 secs.	C_4 (F.)	3 mins.
F (M.)	2 mins. 5 secs.	C_5 (M.)	2 mins. 34 secs.
L (F.)	4 mins. 3 secs.	C_6 (F.)	2 mins.
R (F.)	2 mins. 24 secs.	C_7 (F.)	2 mins. 50 secs.
N (F.)	4 mins. 24 secs.	C_8 (F.)	3 mins.
X (F.)	2 mins. 15 secs.	C_9 (M.)	7 mins. 30 secs.
Average	2 mins. 20 secs.	Average	3 mins. 29 secs.
M.V.	1 min. 6 secs.	M.V.	1 min. 9 secs.

reagents. Only one child, in fact, seemed to get any meaning from the sentence as a whole. A simpler sentence and one in print instead of script would have been a more legitimate test of the ability of children to interpret such reversals. As, however, the children were the fourth group tested it seemed necessary in spite of the defect in method, to finish with the original sentence. The children, a statement that should be emphasized, showed none of the bewilderment so frequently exhibited by adult reagents on being shown the card. They accepted very simply the statement that the

writing was different from that to which they were accustomed and began reading at once. Most of the letters were recognizable at first sight but only the simpler words were read as wholes.

A survey of Table I. shows at once that if a growth factor is significant, it is cut by very great individual differences. The low records, that is those below 60 seconds, occur in every group except the fourth; one in Group I., two in Group III., two in Group II. With such few reagents and such high M.V.'s the group averages cannot be very significant. It appears, however, that the two groups of younger reagents do show records superior to those of the two groups of older reagents both in the lower group average and the lower M.V. Moreover, if we do not include C_9 in our group of children—his general attitude being as said above very different from that of the other children—we get a group average of 2 minutes 59 seconds with a mean variation of only 25.8 seconds.

The median value of these 36 records is 3 minutes 5 seconds. The following table shows the number of reagents in each group whose speed is above and below the median value.

TABLE II

MEDIAN VALUE 3 MINUTES 5 SECONDS

	Below Median	Above Median
Group I	3	6
Group III	2	7
Group II	6	3
Group IV	7	2
Total	18	18

Such a division also points to the superiority of the two groups of younger subjects. In general the results would seem to indicate that age is one of the factors determining speed of reaction but not the only factor.

On account of the element of unfairness in the test of the children, it does not seem worth while laying stress upon the coefficient of correlation of decreased speed and increased age calculated by a ranking of these thirty-six reagents and

utilization of the formula $R = 1 - (6\Sigma D/(n^2 - 1))$, known as Spearman's footrule. $R = .129$, P.E., .071. Such figures are inconclusive.

Group II., as stated above, was enlarged to include 25 college juniors and seniors. This group is fairly homogeneous. The ages range from 18 years 10 months to 26 years 9 months. It was thought that correlation of speed with lesser age might show for this group as a whole. R, under these conditions, is in fact much higher, since it is equal to .331 with a P.E. of .086. We find, that is, what appears to be satisfactory evidence of a correlation between skill in mirror-reading and lower age. It is interesting to note in this connection that the greatest difference between rank for speed and rank for age is, with one exception, found in the case of reagent X. While, that is, X showed little superiority in absolute speed of mirror-reading, she showed considerable comparative speed if the factor of age be at all significant.

B. Sex and Speed in Reading Mirror-script

If we take the four groups of reagents already discussed, including in the second group the twenty-five college seniors and juniors who were tested, we have in all 52 reagents. Twenty-six of these were girls and women; twenty-six were men and boys. The median record for these 52 reagents is 3 minutes 49 seconds. Table III. gives the distribution above and below the median for the sexes.

Table III

Median Time 3 Minutes 49 Seconds

	Below Median	Above Median
Women and girls	12	14
Men and boys	14	12

There is, that is to say, very little difference in the distribution. If any, it is in favor of the men and boys. The three most rapid records were made by young men.

If we take the 25 college reagents, a more homogeneous group, we find the same sort of distribution. There were thirteen young women, twelve young men. One of the

young women made the median record, 4 minutes 3 seconds. The other reagents fall into groups as shown by Table IV.

TABLE IV

MEDIAN TIME 4 MINUTES 3 SECONDS

	Below Median	Above Median
Women	5	7
Men	7	5

Such a result shows that the sex difference reported where breaking an association between visual and motor factors is concerned does not hold for the interpretation of visual reversals. It would suggest that Whipple's explanation of the superiority of girls and women in the former test as due to their greater familiarity with mirrors is a plausible conjecture.

C. *Mirror-reading and Perceptual and Imaginal Predispositions*

In this connection we ask what mental factors condition such varying proficiency in mirror-reading. Various factors are, of course, evident. The ability to utilize context was of considerable aid in interpretation.[1] Reagents showing such ability needed to reverse only a fractional part of the sentence. Furthermore, even within the limits of the test there was evidence of visual habituation for certain subjects. These reagents were able to deal with later letters on the basis of their experience with preceding ones, an ability which other subjects did not show, as they repeated the original process of interpretation even on letters occurring several times.

Emotional factors also complicate the situation. In this respect Groups I. and II. had the advantage, since these reagents were well acquainted with the experimenter and, in most cases, had had considerable experience in serving as reagents in psychological tests.

The reagents who read the script most rapidly reported

[1] The utilization of meaning in mirror-reading, tested later by correlation of such a test with results from a completion test, appears to be much less than I should have anticipated, a fact which enhances the value of mirror-reading as a test of specific ability.

that many words were significant as they stood without need of reversal. Subject *B*, who was very expert in mirror-reading and practiced in observation, reports that he notices a curious variation from day to day in the degree to which mirror-reversals appear immediately significant. This report suggests, again, the need of a study of the factor of habituation.

The most plausible conjecture with reference to variation in the speed with which mirror-script is read is that it is dependent upon varying dispositions in the utilization of motor and visual material. Stern conjectured that the child visually preoccupied is more apt to produce visual reversals than the child whose motor activities bring him into intimate contact with space relations. It is natural, then, to ask whether a grouping of reagents as predominatingly visual or motor would have significance in connection with skill or maladroitness in dealing with sensory reversals. So far attempts to answer this question have proved abortive.

The reading of mirror-script was utilized in my monograph on control processes in modified handwriting in the hope of throwing into the foreground certain individual methods of procedure which might be correlated with the control processes used by the same subjects in the writing of mirror-script. A sentence may be quoted from that report to show the general result: "In general, the test had little bearing on the topic under consideration other than showing that the greater mental readiness and facility in adaptation shown by certain subjects in the more distinctly motor tests is paralleled by the result from this test, which is sensory in nature."[1] My conclusion that the striking individual differences evident in the ease with which different reagents did mirror-reading is to be attributed to individual variation in adaptibility was vague in the extreme.

My purpose in the monograph tests was to determine what kind of imagery is used in interpreting and producing mirror-script. Miss Fernald[2] has sought to determine

[1] *Op. cit.*, p. 39.

[2] Fernald, M. R., 'The Diagnosis of Mental Imagery,' Psy. Rev. Mon., 1912, XIV., No. 58.

whether mirror-reading produces a tendency to use any one particular kind of imagery. She says briefly of these tests: "The difficulty or ease did not seem to be correlated with the kind of imagery used." And again: "In general, we may say that this procedure tends to throw the auditory-vocal-motor combination into prominence and to suppress the visual."[1]

One today speaks of imaginal types with considerable diffidence. Not only are we embarrassed in our general conception of sensory predispositions, but also, in the present instance, we do not know whether it is important to lay stress upon the specific method utilized in handling the situation or upon general sensory tendencies. Furthermore, it may well be that the spatial situation in course of development is very different from that found after a mode of reaction has been established. We are, then, thrown back upon necessity of a genetic treatment. Such a treatment lies beyond the scope of the present paper.

A few reports upon the imaginal reactions of the subjects of Groups I. and II. may, however, be included. In all imagery work negative results appear to be particularly significant, where, for example, extensive experimentation with a given subject fails to arouse a specific form of imaginal reaction. Thus, Subject I. (Group I.) as shown by both my work with her and by subsequent tests by Miss Fernald is very defective in visual imagery. She is, also, the poorest, with perhaps one exception, of the seventy-nine reagents whose records I have for the reading of mirror-script.

In Group II., reagents *E* and *F* also show striking deficiencies in visual content. Subject *E*, who employs motor content extensively, is thirty-ninth among the seventy-nine subjects. She was able, however, to utilize meaning to a rather unusual degree. Subject *F*, whose visual bankruptcy is caused probably by extremely poor eyesight, showed considerable skill in deciphering mirror-script but such skill was dependent upon very clever trying-out, in a vocal-motor

[1] *Op. cit.*, p. 47.

way, of one letter after another, and not upon immediate recognition of forms. On the other hand, reagents *C* and *R*, both of whom were good at mirror-reading, show striking preoccupation with visual material in a very great number of situations, *T* being accustomed to translate even auditory content into visual form. *X*, the subject of the report of the first section, also showed strong visual tendencies. *B* had excellent command of a number of imagery forms, including the visual and motor. On the whole, visual preoccupation appears to favor mirror-reading.

§ 4. The Writing of Mirror-script

A consideration of the varying relationship of visual and motor elements in the test under discussion raises naturally the question whether ability to read mirror-script is correlated with ability to write it and the converse. It might be anticipated that there would be no necessary relation between the two, so that a reagent might be much more successful in either the sensory or the motor test. The test to be reported relates, again, only to a first trial and does not, therefore, give evidence as to the important factor of habituation. It was planned to determine the extent of correlation of mirror-script reading and writing, first trial. Two further questions were considered, first, to what extent the ability to read and write mirror-script (first trial) correlates with the ability to read and write inverted script (first trial) and, second, to what extent the mirror-writing of right and left hands correlate.

The reagents for this test were the twenty-five freshmen girls mentioned in the preceding section. These girls were taken from the writer's first-year psychology class and were selected simply by the chance agreement of their free hours with those of the experimenters.

Method.—As a preliminary each reagent was asked whether or not she had practiced the writing of mirror-script, whether she herself showed any left-handed tendencies, whether she possessed any left-handed relatives. The reagent was then timed in the writing, with the right hand, an alpha-

betical sentence of forty letters which had previously been memorized. Secondly, the reagent wrote this same sentence on a second slip, beginning at the right-hand margin of the paper and writing mirror-fashion. Thirdly, the reagent was required to memorize a second alphabetical sentence, also consisting of forty letters and was timed in writing it on a third slip with her left hand, in the normal fashion. Fourthly, the reagent wrote this same sentence with her left hand on slip 4, beginning at the right-hand margin of the paper and writing mirror-fashion. The fifth test was the reading aloud of the sixty-letter sentence written in mirror-script, utilized in the test of the preceding section. Test sixth consisted in writing with the right hand in inverted script on slip 5 the sentence first memorized; test seven consisted in reading a sentence of sixty letters written in inverted script. In every test the speed of reaction was timed.

In the series as planned, the left hand got the benefit of any habituation that occurred from a preceding trial in mirror-writing. There was, also, a slight chance for such habituation to affect the speed of mirror-reading.

Results.—Some general statements may be of interest before an interpretation of particular results is undertaken. The time-range in this group for the mirror-reading (sixty letters) is from 25.4 seconds to 21 minutes 15 seconds; the median value is 3 minutes 23 seconds. The time-range in writing mirror-script (40 letters) is from 45 seconds to 4 minutes 43 seconds; median value, 2 minutes 25 seconds. It would seem, then, that in many cases mirror-writing is less difficult than mirror-reading.

The reading of the sentence written in inverted script (60 letters) ranged in time from 8.2 seconds to 3 minutes 58 seconds; median value, 22.5 seconds. The reading of inverted script is obviously much easier than either the writing or reading of mirror-script. This result agrees with the report of Dearborn that, under conditions of objective reversal, an inversion has less influence upon recognition than any other shift in position.

The speed of writing inverted script ranged from 1 minute

50 seconds to 6 minutes 56 seconds; median time, 3 minutes 13 seconds. Such writing is much more difficult than the reading of inverted script. It is somewhat more difficult than the writing of mirror-script. Again, such a result suggests agreement with Meyer who found in the reproduction of figures that mirror-reversals occurred more frequently than other reversals. It appears, then, that while visually an inversion is more easily handled, the mirror-reversal is more easily produced 'motorly.'

The various coefficients of correlation for the different tests are given in Table V. These coefficients also were calculated by the use of Spearman's footrule for finding *R*. In the first utilization of material, the rank is calculated purely on the basis of speed in writing. Later, the matter of quality of writing will be discussed.

TABLE V

SPEED OF WRITING. RAW CORRELATIONS (*R*)

25 Subjects. P.E. .086

	Reading Mirror-script	Writing Mirror-script, Right Hand	Writing Mirror-script, Left Hand	Reading Inverted Script	Writing Inverted Script, Right Hand	Writing Normal, Left Hand
Reading mirror-script	....	.485	.173	.452	.264	.081
Writing mirror-script, right	.485	....	.413	.456	.490	.062
Writing mirror-script, left	.173	.413	....	.355	.384	.264
Reading inverted script	.452	.456	.346	....	.350	.187
Writing inverted script, right	.264	.490	.375	.350	....	.149
Writing normal, left hand	.081	.062	.264	.187	.149	....

Before proceeding to a discussion of the correlation coefficients, it may be well to recall that, according to Spearman, "*R* has no scientific significance—except negatively—unless it be at least twice as great as its probable error. . . . For a perfectly satisfactory demonstration, the *R* should be not far short of five times greater."[1] Again, "One sees that when a high correlation (say $R = 0.50$) exists between two things, its existence may be satisfactorily proved by—as a minimum—about a dozen cases. But, on the other hand, it is clearly no good attempting to demonstrate a low corre-

[1] *Op. cit.*, p. 96.

lation (say $R = 0.20$) without first collecting for this purpose at least a hundred cases. And, similarly, about 100 cases would be required to prove that no correlation over 0.20 existed. While to prove with certainty the absence of any correlation over 0.02 one would need 10,000 cases."[1]

Inspecting the table with these limitations in mind, we find there is very satisfactory evidence of the correlation of the sensory tests with the motor. R for mirror-reading and mirror-writing, right hand, is .485, a value more than five times its P.E. Reading and writing inverted script correlate less closely, R in this case being only a trifle more than four times its P.E. The two sensory tests (the reading of mirror and of inverted script) show a high correlation, .452; the two motor tests (the right-hand writing of mirror and of inverted script) give an even higher correlation, .490.

These results suggest some interesting questions. The most interesting are those which relate to the left-hand correlations. Thus, mirror-writing with the left hand shows a very low correlation with mirror-reading, although a high correlation with right-hand mirror-writing. A reference to Table V. shows also a fairly satisfactory correlation for left-hand mirror-writing and the reading and writing of inverted script, and also for speed of normal left-hand writing. The reduced and by no means satisfactory correlation of left-hand mirror-writing and mirror-reading may be due to the factor of habituation, since the left hand had the advantage of the right hand practice. Of this, more later.

The questionable correlations of the speed of the left-hand normal writing with either the reading of mirror-script or the writing of it with the right hand is interesting, since according to a traditional view there is thought to exist some relationship between lefthandedness and skill in producing mirror-script.

Another traditional view holds that mirror-writing is the normal writing for the left hand in general. We may ask, then, how mirror-writing with the left hand compares with the normal script produced by that hand and how it com-

[1] *Op. cit.*, p. 97.

pares with right-hand mirror-writing. As before the comparison is first made on the basis of speed only. The time required for writing left-hand mirror-script ranged from 1 minute 5 seconds to 3 minutes 30 seconds, median value, 1 minute 50 seconds. The time required for normal left-hand writing ranged from 25 seconds to 2 minutes 45 seconds, median time, 1 minute 15 seconds. Left-hand mirror-writing is somewhat slower than the unreversed left-hand writing. Left-hand mirror-writing is, however, more rapid than right-hand mirror-writing under the conditions of the test. How far, we may ask, is such increased rapidity due to habituation?

The following facts are pertinent. Six of the reagents, in spite of the habituation factor involved in the second or left-hand test, gave a more rapid record for the right than for the left hand. The other nineteen subjects gave a more rapid record for the left hand. This gain in rapidity varied from 2 minutes 51 seconds to 10 seconds. Reference to the individual records shows, however, that the extreme gain, nearly one and one-half minute more than the next highest, was made by a naturally left-handed reagent, who employs her right hand in writing only because of definite insistence upon its use when she learned to write. Furthermore, the six reagents who stand next in order as showing the greatest gain in rapidity for the left-hand mirror-writing are, with one exception, subjects who reported that they possessed left-handed relatives. The third highest record for increase is, indeed, made by the sister of the left-handed reagent who showed the greatest increase. Possibly, then, these subjects brought to the left-hand mirror-writing not only some practice but also some latent skill in the use of the left as compared with the right hand. The reagents in the middle range show a gain in speed from 10 seconds to 51 seconds, a gain quite within the range of possible habituation. The average increase of the whole group, omitting the one left-handed reagent and the six reagents whose right-hand records were more rapid than their left-hand ones, is 44.6 seconds with a M.V. of 22.7 seconds.

In the monograph tests already referred to, in those cases where the left-hand mirror-script was written first so that the right hand had the habituation advantage, the right hand, although with exceptions, was more rapid than the left.

It does not appear, then, that the left hand is, in general, more proficient than the right in mirror-writing in both right- and left-handed individuals.

Approaching the subject in another way, we may take the reagents in the order of their increased rapidity of left- over right-hand mirror-writing and work out the correlation coefficients (1) for rapidity left-hand mirror-writing and (2) for rapidity normal left-hand writing. We find that R in the first case is .024 or practically, 0; in the second case it is .307. That is, the gains in the left-hand mirror-writing correlate not with absolute rapidity in left-hand mirror-writing but, rather, with the rapidity of normal left-hand writing. This would seem to indicate that latent ease in using the left hand enters as a factor determining the speed of habituation.

We find then, that habituation entered into the second mirror-writing test and that this habituation appeared to be greater in the case of reagents with left-handed tendencies. Do these facts account for the lower correlation of left-hand mirror-writing with mirror-reading in comparison with its correlation with right-hand mirror-writing?

Before answering this question, let us compare the mirror-reading skill of the six reagents who—as shown by the fact that their right-hand mirror-writing is more rapid than their left-hand mirror-script—are the most right-handed of the group, with the six who show the greatest gains in the left-hand test. The ranks of the first six for mirror-reading are as follows: 10, 1, 5, 2, 4, 3. The ranks of the second six are as follows: 24 (the left-handed reagent), 15, 23, 25, 11, 16. Such a grouping seems to show that *not habituation merely is responsible for the low correlation of left-hand mirror-writing with mirror-reading but that degree of righthandedness is itself a factor in the high correlation of mirror-reading and right-hand mirror-writing.*[1]

[1] R for increased rapidity of left- over right-hand mirror-writing and mirror-reading is $-.15$. R for relative superiority of right-hand mirror-writing and mirror-reading is .394.

Let us now consider these same questions with reference to the quality of writing produced. With such a shift in the standard of comparison we are, of course, introducing a greater chance for error than when using rapidity of work as the basis for ranking reagents. There exists no scale for the rating of left hand or mirror-writing, the available writing scales not being adequate for the measurement of such writing. In order, therefore, to obtain some estimate of the comparative proficiency of different reagents in the quality of their left hand and mirror-writing, five of the experimenters who had assisted in the tests made one serial arrangement each of the twenty-five specimens, for right- and left-hand mirror-writing and for the normal left-hand writing. The slips of paper used were transparent so that it was possible by reversing them to pass judgment upon the mirror-writing under the usual conditions. The average rank and the M.V. for each specimen was then calculated.

The averaged ranks for the left-hand normal writing ranged from 1.2, with a mean variation of .32, to 24.4, M.V., .48. The average M.V. for the twenty-five specimens was 1.92.

The averaged ranks for the left-hand mirror-writing ranged from 2.8, M.V., 1.36, to 24.2, M.V., .64, with an average M.V. for the twenty-five of 2.37.

The averaged ranks for the right-hand mirror-writing ranged from 1.2, M.V., .32, to 24.4, M.V., .96, with an average M.V. for the twenty-five of 2.016.

The qualitative differences for the left-hand normal appear to be more distinct and more easily graded than such differences in the case of mirror-writing with either hand. Right-hand mirror-writing is, however, more easily graded than left-hand mirror-writing.

A third comparison was made, that is, the right- and left-hand mirror-writings of each reagent were compared by three of the judges, chosen so as to include the judge who was least and the one who was most representative of the group as shown by their average mean variation from the average for the group. The judgments of the three judges

were very uniform. Right-hand mirror-writing is, with a few exceptions, of a perceptibly higher grade than is left-hand mirror-writing, in seventeen out of the twenty-five cases. In five other cases the judges agree on the superiority of the left-hand mirror-writing; for three specimens only is there some disagreement as to whether right or left mirror-writing should be marked superior in quality. A reference to the original records shows that the eight subjects forming the second group, whose left-hand mirror-writing excels or equals in quality that of the right hand, are, with two exceptions, those who possess left-handed relatives. The better quality of the right hand mirror-writing would seem to indicate that there is no natural tendency for the left hand to excel in such reversal except in so far as there is a latent tendency to left-handedness.

One asks, further, whether the left-hand mirror-writing is qualitatively superior to the normal left-hand writing as one might expect if the traditional view is correct that mirror-writing is the normal writing for the left hand. The three judges who had compared right- and left-hand mirror-writing also compared the left normal and the left mirror-writing of every reagent. The results were less uniform than in the preceding case. In eleven cases, however, they agreed upon the superiority of the left-hand normal writing and in one case upon the superiority of the left mirror. This last writing was produced by the one left-handed reagent. There were six other cases in which the judges agreed that the left-mirror was equal to or a trifle superior to the left normal. In the six remaining cases there was disagreement, with two of the judges agreeing in four of these cases that the left normal was superior to the left mirror. So far as the evidence goes it seems to point to a more frequent superiority of the left-hand normal although this superiority is less common than is the superiority of the right-hand mirror-writing.

Coefficients of correlation with a ranking based on qualitative differences were worked out and are given in Table VI. The chances for error are, of course, very great. Even so, the figures are suggestive.

TABLE VI

QUALITY OF WRITING. RAW CORRELATIONS (R)

	Quality			Speed			
	Mirror-script, Righ	Mirror-script, Left	Normal Script, Left	Mirror-script, Right	Mirror-script, Left	Normal Script, Left	Mirror Reading
Right mirror	......	.524	.225	.062	......	......	.278
Left mirror	.524	......	.370	......	−.129	......	.149
Left normal	.225	.370	......	......	......	−.048	−.096

From this table we see that on the basis of quality, as well as of speed, right-hand mirror-writing correlates more closely with mirror-reading than does left-hand mirror-writing. The quality of left-hand normal writing does not correlate with mirror-reading. In general, the lack of correlation of speed and quality is instructive and should make possible interesting comparisons if we had records from the subjects as to kind of sensory control utilized in the test.

§ 5. SUMMARY AND CONCLUSIONS

The first section of this paper reports a case of spontaneous right-hand mirror-writing which presents points of interest in that even in the adult state the subject shows a tendency to revert to this form of writing and gives evidence of maintaining mirror-reversals in her imaginal representation of words. The mirror-writing seemed conditioned by a general difficulty in orientation which from the social side showed itself in some inadequacy in dealing with the meaning-aspect of experience. Practically, the reagent found herself skilful in type-setting with subcapacity in the interpretation of varied form, such as that found in illegible writing.

The use of other reagents in a control series of tests (section three) to find the reagent's comparative skill in interpreting mirror-reversals showed that there were college students who excelled X and revealed striking individual differences in the ability to deal with such reversals. The literature of the subject (section two) suggested that possibly age might be the controlling factor in such varying capacity since spatial position appears to be an outcome of the fusion of the visual and motor elements of experience. The evidence

at hand points strongly to growth as one of the determining factors but indicates, also, the presence of other factors. Sex did not appear to be one of these factors. It seems plausible as a conjecture that capacity to interpret mirror-reversals is dependent upon visual as against a motor preoccupation.

A fourth section of the paper sought to determine the range of variation in skill in mirror-writing among twenty-five freshmen and to determine how far such skill correlated with skill in mirror-reading. The correlation was found to be high with the right hand, much higher than with the left. Apparently the results, in contradiction to the traditional view, point to a correlation of degree of right-handedness (or, possibly, degree of specialization of function) and efficiency in mirror-reading. This is what one might expect if specialization of function goes with increased adequacy in orientation.

But have we any reason to believe that efficiency in mirror-reading is in any way related to adequacy of spatial orientation in general? We are left, moreover, with certain puzzling questions on our hands. How account for decrease in skill in interpretation of spatial reversals with age if such skill be correlated to any degree with increased adequacy in orientation? How reconcile the conjecture that efficiency in mirror-reading may be due to visual preoccupation with the apparent correlation of such efficiency with right-handedness?

Obviously, the tests just reported are insufficient to answer these questions. More extensive tests are called for. Specifically, one should utilize the various tests of the index of righthandedness. One should determine whether such a thing as degree of righthandedness correlates with age. If possible, some classification of reagents into visual and motor should be attempted. Above all, the effect of habituation should be studied for quite possibly this may be found to result differently in the visual test (mirror-reading) from what it does in the motor test (mirror-writing).

Certain of these tests the writer hopes to carry to some conclusion.[1]

[1] I wish to express my thanks to the Misses Foster, Eby, and Johnson, and to Mr. John Hill and Mr. John E. Anderson for their aid in gathering data for the above report.

A COMPARATIVE STUDY OF RECOGNITION AND RECALL[1]

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During the last several years McDougall,[2] Calkins,[3] Strong,[4] Hollingworth[5] and others have pointed out the fact that there is a decided difference between recognition and recall memory. The attempts to measure this difference, however, have been rather meager.

The following study was started in the winter of 1910–11 when the writer was gathering data for his 'Study in Incidental Memory.' 109 boys and 125 girls of Royersford, Pa., from the high school to the fourth grade inclusive had been asked to spell some words. Six words were pronounced as for a spelling test and after one half hour the subjects were unexpectedly asked to recall the words. After 3 months the subjects were again surprised by a request for a second recall. After two minutes had been given for recall, and papers had been collected the writer pronounced the six words, distributed by chance, among 10 other words. The children wrote these words as they were pronounced and were then asked to mark the words they recognized.

To the surprise of the writer, while not one of the 234 recalled 6 words and 55 recalled none, only 1 failed outright to recognize any of the 6 words, and 62 subjects,—32 boys and 30 girls recognized all 6. Of these 32 boys and 30 girls who recognized all 6, 23 boys and 25 girls had no wrong ones.

[1] Reported before the Conference on Individual Psychology held at Columbia University, April 6–8, 1914.

[2] McDougall, R., 'Recognition and Recall,' *J. Phil., Psychol. and Sc. Methods*, 1904, Vol. 1, pp. 229–233.

[3] Calkin, Mary Whiton, 'A First Book in Psychology,' Ch. VIII., pp. 124–132.

[4] Strong, Edward K., Jr., 'The Efficiency of Length of Series Upon Recognition Memory,' Psychol. Rev., 1912, Vol. 19, pp. 447–462.

[5] Hollingworth, H. L., 'Characteristic Difference between Recall and Recognition,' *Amer. Jour. Psychol.*, 1913, Vol. 24, pp. 533–544.

In as much as those who recognized all six words were not measured to the extent of their ability, and therefore since the total percentages for recognition were naturally too low, the writer did not venture to include the records in the 'Study in Incidental Memory.' The works of Hollingworth,[5] and Strong[4] which have appeared in the meantime stimulated a continuation of the study.

Having tested with 10 words 350 children of the public schools of Tyrone, Pa., on the influence of recall in retention, about a year ago, the writer returned ten months later for a recall and recognition test. He gave the 10 words which had been previously given along with 10 other words for the recognition test.

It is obvious here that because of the nature of the tests the same experimenter can test only a limited number in a place, for no valid tests in incidental memory can be made where the subjects have a knowledge of the nature of the test beforehand.

With 20 words as stimuli and with these words distributed by chance among 20 other words, tests were made in the public schools of three other towns (Mt. Union, Bridgeport, and Conshohocken, Pa.). For one town (Tyrone) one week intervened between the presentation of the stimuli and the first and only recall and recognition. In the two neighboring towns (Bridgeport and Conshohocken) only one day intervened. In these three schools Mr. Raymond Ellis a junior in the writer's class in psychology in Juniata College was experimenter. His acquaintance with the respective superintendents and many of the teachers contributed toward the success of the experiment.

The superintendents, teachers and experimenter were provided with printed copies of the directions. The experimenter pronounced the stimuli words and the words for recognition. Each word was pronounced only once and the speed was determined by the time when the pupils looked from their papers. The teachers and superintendent aided in giving the directions and in the prevention of cheating. The teachers as well as the children, however, were left

under the impression that it was a mere spelling test. In all, 687 subjects,—333 boys and 355 girls were tested.

Since the same subjects were tested and with the same materials for recognition and recall, and since the recall test was always given before the recognition test, two assumptions were obvious, namely, that any word which could be recalled could always be recognized afterwards and that no word could be recalled which could not first have been recognized. A study of the Royersford group shows that the first assumption is well grounded, for only three of the 234 subjects recalled words (one word each) which they did not afterwards score as having recognized. The probability is that these 3 merely failed to see these words when they checked up for recognition, due to haste or emotional disturbance. This fact is equally obvious in the other groups. No way presented itself in this study to readily test the second assumption.

Total averages for recall, and for recognition without regard to chance, were computed in terms of words and per cent. The comparison emphasized most is between recall and efficiency of recognition. The latter was computed from the averages of all the individual percentages by Strong's formula[4] $C/T \times ((C - W)/(C + W)) \times 100$ per cent., for by all the groups but one in the words for recognition, since the same number of stimuli words as other words obtained, as many correct as incorrect words would be marked by mere chance. For the group where ten words were added to the six stimuli words this formula was modified thus: $C/T \times ((C - W \times .6)/(C + W \times .6)) \times 100$ per cent. A comparative study was also made of individual records, as well as a study of the correlation between recall and recognition. Likewise tables of frequency for recall and recognition, along with the average deviation from the averages were computed.

Table I. gives the total averages by the four groups tested for recall and recognition in terms of words and per cent., the ratio of recognition efficiency to recall, and the average group-gain of recognition over recall. Beginning at the left one reads:—"males of B. and C. schools on the average

recalled 4.5 words or 22.9 per cent. of the whole number of (20) words given, with a variation from the average of 8.9 per cent. The same group have an average efficiency for recognition (according to above formula) of 66.5 per cent. with an average deviation from the average of 14.6 per cent. On the average, without considering those scored for recognition by mere chance, 16.5 words were recognized or 82.5 per cent., with an average deviation of 10.7 per cent. The ratio of recognition efficiency over recall is 2.9. The gain of average recognition efficiency over recall is, for this group, 43.6, and 12 more words were recognized on the average than were recalled." As would be expected the highest averages for recall and recognition obtained after one day's interval and the lowest after ten months, with a considerable gain after one day, over recall after one week. However, it must be remembered that the Tyrone group (ten months' interval) was given only ten words while the one-day and one-week groups were given these same ten words with ten more, and the three week group had been given only six words. Therefore a fair comparison for effect of time interval can be made only between the first two groups. The variability for recall runs from 6 per cent. to 19 per cent. or .6 to 1.9 words and is decidedly less after one week than after one day. The highest variability obtains for the three week recall for six words.

The average per cent. for efficiency in recognition and for total correct recognition take the same general trend as those for recall. However, the variability is almost constant for efficiency of recognition, save for the Royersford group where it is proportionally high. The variability fluctuates somewhat more for total correct recognition, but the total variability for Royersford is not proportionally high. While the absolute variability for recognition is considerably higher than that of recall the relative variability in terms of the respective amounts recalled and recognized is much lower for recognition than for recall. The gain of recognition over recall in per cent. and in words is shown in the last two columns. They show a higher increase for one day recall than for one week.

With no regard for records by chance, the individual gains of total recognition over recall are shown in the following tables of frequency:

Gain of Total Recognition over Recall in Words

No. Words	B. and C. (20)		Mt. Union (20)		Tyrone (10)		Royersford (6)	
	1 Day		1 Week		10 Months		3 Weeks	
	M.	F.	M.	F.	M.	F.	M.	F.
17	7	1	....	1	....	....	....	...
16	6	2	2	3	....	....	....	...
15	11	4	7	3	....	....	....	...
14	11	9	3	8	....	....	....	...
13	19	20	7	8	....	....	....	...
12	25	21	6	3	....	....	....	...
11	15	16	3	10	....	....	....	...
10	6	17	6	7	....	....	....	...
9	9	8	2	6	1	1	....	...
8	7	6	4	7	3	2	....	...
7	2	3	1	2	17	11	....	...
6	2	3	....	2	14	16	2	4
5	2	....	3	2	6	9	17	7
4	....	....	5	2	7	7	24	15
3	....	....	1	....	2	7	29	44
2	2	....	....	....	....	2	24	27
1	....	....	....	....	....	....	9	7
0	....	....	....	....	....	....	4	18
....	....	....	....	....	....	....	....	3
Ave.....	12.0	11.3	10.6	10.9	6.0	5.4	3.1	2.5

The figures in parenthesis indicate the number of stimuli-words used. Under "M." of Mt. Union one reads, for example, "Two boys scored 16 more words in recognition than they recalled; seven, 15 more; three, 14 more, etc. The gain of total recognition over recall is about one half the number of stimuli-words used for the respective groups. A study of the B. and C. group shows a slight increase in average gain of total recognition over recall, from the lowest grade to the high school. By the boys the gains are, 10.8 words for the 5th and 6th grades; 11.4, for the 7th and 8th grades; and 14.2 for the high school. For the girls, the records are 10.8, 11.0 and 15.6 words respectively. On the other hand, the Mt. Union group has the lowest gain for the 7th and 8th grades, and the lowest grades are about as high in their gain as the highest. In order of the figures for the

B. and C. groups, the respective gains for the boys by Mt. Union are, 10.8, 9.7, and 11.1 words; for the girls, 11.4, 9.5, 11.9 words, respectively. Out of 105 cases tested after 10 months, 103 cases had more correct than incorrect answers for recognition, and therefore, made more than a zero record in recognition.

For all the groups the girls greatly excel the boys for recall. There is a small superiority of girls over boys for total correct recognition, but a slight exception obtains for this rule of sex difference in efficiency of recognition: the ratio of recognition to recall, the gain, by the girls, of per cent. recognition efficiency over recall, and of average number of words correctly recognized over those recalled show the boys strikingly superior to the girls. The same holds true for all groups throughout the grades (Table II, not printed). This indicates that while the girls are a little superior to the boys in proportion to their efficiency in recall they are inferior to boys in recognition. For recall the girls show more variability than boys, while for recognition the opposite is true.

A comparison by grades Table II. reveals the same superiority of girls, for recall for every grade of every group. However, no rule holds true for sex difference for recognition efficiency nor can any rule for sex difference for variability be formulated for recall or recognition. Furthermore the greater gain by the boys than by the girls in recognition over recall holds true by grades almost without exception, as a little subtraction in Table III. will show.

Table III. contains the tables of frequency of the groups by grades combined for correct and for incorrect words recalled and recognized. For example one reads from first column to the left under "M." of B. and C.: 2 subjects recalled 12 words, 1, 11 words; 1, 10 words; 3, 9 words; 4, 8 words, etc. At the foot of each column is the average number of words recalled and recognized. These averages show girls superior to boys for recall, as one can readily see also by frequency table; but for incorrect recall the girls stand slightly higher while in all the groups the boys recognize more wrong words than girls. There is a general increase

for both sexes in incorrect answers and decrease of correct answers with increased period of time.

Table IV. gives the percentages of all answers given that are correct. With the exception of the group with one day interval more answers are correct for recognition than recall and the superiority of the recognition answers over those of recall increases with increase of time. It would seem that one's recognition memory after a long interval of time is much more reliable than one's recall memory.

	Mt. Union				Tyrone			
	Time 3 Wks.: 20 Stimuli-words				Time 10 Mo.; 10 Stimuli-words			
	Recall		Recognition		Recall		Recognition	
	Correct	Incorrect	Correct	Incorrect	Correct	Incorrect	Correct	Incorrect
1	5	0	15	1	1	1	6	4
2	1	4	16	4	1	0	8	2
3	0	0	15	6	0	0	9	1
4	1	0	17	8	1	2	5	3
5	4	1	17	4	0	3	7	2
6	4	4	16	2	0	2	3	3
7	3	0	15	4	1	1	8	2
8	3	1	17	3	1	0	5	2
9	1	0	11	2	1	5	5	5
10	1	1	13	1	0	1	7	3
11	3	1	11	1	1	1	6	4
12	1	1	16	10	2	4	8	2
13	3	0	14	8	3	2	7	2
14	4	0	17	5	2	1	7	1
15	5	0	18	2	3	1	6	3
16	4	1	13	3	4	4	6	4
17	3	1	18	5	0	0	4	1
18	3	1	16	2	2	2	9	1
19	2	1	10	0	2	1	6	4
20	5	5	12	1	0	4	7	3

A correlation between recall and recognition by the Royersford group gives, by Pearson's method, for the boys, .596; girls, .304. By the method of unlike signs the correlations are .535 and .308 respectively. Since these two methods gave such like results the latter method only was used for the other groups. For the boys in the B. and C. group the figures are +.209, for the girls .517 and in the Mt. Union group they are −.209 and 0 for the boys and girls respectively, for Tyrone the correlations are .011 and .468. Aside from the Royersford group where such a large

number made perfect records in recognition, the correlation for the girls is superior to that for the boys. All the correlations are remarkably low for all groups and for one group is practically absent. Perhaps these figures reveal a bigger difference between recognition and recall memory than any other figures.

Twenty records selected at random from each of the Mt. Union and Tyrone groups are given above.

It is an interesting fact that an individual who recalled no words recognized 15 and another who recalled 5 recognized only 15. Random sampling from any of the groups show many examples as striking as these. It is evident that what has apparently been forgotten outright is really clinging somewhere in the field of consciousness.

As pointed out by Kirkpatrick[1] and by Myers,[2] many of the incorrect words given show a high degree of association in form or in meaning, with the stimuli-words. This holds true for both recall and recognition, and for all the groups with the various time-interval and stimuli. General ideas which seem to have been called up when the words were given are evidently carried over. In the Royersford group most of the stimuli words (though not purposely so) refer to a house or household effects and are especially suggestive of kitchen and attendant associations. They are: angel, pickle, dirt, busy, onion, women. As a result, both for this group and for the Tyrone grades, tested with these same six words, along with four more stimuli-words, one finds that among the incorrect words 64 per cent. of the answers seem to be associated with a house in general, with edibles, or with culinary objects. The following are some suggestive words of this type, taken from the combined records of the above-named groups:

7 dish	4 basket	3 coal	1 dust
6 bucket	4 pitcher	2 stove	
5 kitchen	4 broom	2 pan	

[1] Kirkpatrick, E. A. An Experimental Study of Memory, Psychol. Rev., 1894, 1, pp. 602–609.

[2] Myers, G. C. A Study in Incidental Memory, Arch. of Psychol., No. 26, 1913, pp. 64–65.

The figures to the left indicate the number of subjects who gave the respective words. Four subjects gave "wagon," 3 of whom gave it along with "tongue," and 11 gave words with "oo" in them. It is worthy of note that none of the incorrect words are long, but they tend to approach the length of the stimuli-words. No sex difference obtains save that 6 girls named "dish" while only one boy named it, and 4 girls and no boys named "pitcher" among their incorrect recall. On page 64 of "Incidental Memory" the writer has given a list of generally suggestive words, selected at random, for the Royersford group. A complete list is also given for the same group after an interval of one-half hour.

In the group tested after one week, for recall of ten words, the following significant terms were given:

5 horse (donkey)	1 lead (iron)
4 sugar (grocer, pickle, or lemon)	3 candle (lantern)
4 flower (blossom)	2 string (spool, grocer)
3 garden (dirt, shovel)	4 school (spool)
3 mineral (iron)	3 apple (pickle, kettle)
2 copper (iron)	3 monkey (donkey, money)

Likewise in recognition certain words prove to be similar in form or meaning.

For example "picture" which stood first in the list presented for recognition test, was mistaken for "pickle" (obviously) by 87 out of 105 pupils.

After ten months the comparative order of frequency for each word by recall and recognition is shown by the following:

Recall	Recognition	Original Order (correct)
29 shovel	90 shovel	shovel
20 kettle	88 kettle	tongue
18 onion	79 onion	spool
17 spool	77 grocer	kettle
16 pickle	77 spool	feather
11 clock	72 dirt	clock
8 dirt	71 tongue	pickle
4 tongue	67 pickle	dirt
3 feather	64 feather	grocer
2 grocer	43 clock	onion

It might be noted that in the test on the Tyrone group the writer by mistake told the subjects to recall "as many

of the *ten* words," which he had pronounced for them about ten months before as they could. In all the other tests the number of words was not indicated. Three fourths (72 per cent.) of the Tyrone group marked just ten words; while for about the same number of subjects tested with twenty words after three weeks, only 13 per cent. marked the correct number of (20) words as recognized. This indicates that the recognition records for the Tyrone schools are proportionally higher than the recognition for the other groups.

The writer suggests that a study of the effect upon the subjects of their knowing the amount of stimuli to be recalled or recognized would be worthy of a careful study: for example if one has to go shopping for several articles a memory of the number of articles to be gotten will help assure none being missed. This gives one a general scheme and a definite destination in his struggle to remember, and surely in recognition it will tend to make one weigh more carefully the materials from which the familiar stimuli are to be selected.

To the most casual observer the affective element is tremendously more expressive in the process of recognition than in that of recall. It was necessary in all the recognition tests to warn the subjects against uttering exclamations when the word they chanced to recognize was pronounced, for many wanted to laugh aloud or make various types of happy expressions when such words were pronounced. In the movement of limbs and body, and especially in the smiles and facial expression one could almost determine how many words in the recognition list were really familiar to each individual. Certainly the affective side of recognition has not been duly emphasized.

Calkins,[3] Allin,[1] and a number of others observe that a feeling of pleasure generally or always attends recognition, but they give to this phenomenon of recognition only a minor place. The writer maintains that the pleasurable feeling frequently and perhaps always, precedes conscious recognition. A horse sold by his master to a neighbor, will neigh for years when passing the old barn, and will look toward his old home with movements that indicate pleasure.

[1] Allin, Arthur, Recognition, A. J. Psychol., 1895–6, Vol. 7, pp. 240–373.

The dog will make unmistakable movements of happiness when an old friend of his returns home. Certainly no conscious association of ideas is here; yet none would doubt the expressed feeling of recognition. The writer's baby at five months of age after being absent for a day or two from the room and bed to which he was accustomed, kicked and smiled and made extraordinarily numerous movements when again put on his bed.

The writer may not be a good subject but a number of records from his own experience seem to emphasize this affective factor of recognition. Following are a few of these records:

I met a Mr. M. in town H. at Christmas time. At first sight I had a feeling of acquaintance, and at various times since, on referring to him or on hearing his name mentioned I experienced the same feeling: I had no consciousness of any other individual nor did I once have the feeling "He reminds me of some one." On the evening of May 20 following, M.'s image suddenly came to me and with it the image of Mr. F. Then I discovered at once in F. the source of this long-continued feeling.

A certain student (Miss L.) was first in my class February 2. The feeling I had on seeing her then and on almost every day following when I saw her in the class, was one of "at-homeness": but not once did it occur to me that she reminded me of Miss M. However on June 11 of the same year I caught a faint glimpse of a lady on the street, who called up the Miss M. Immediately Miss L.'s image came to me and I had discovered the cause of the feeling. It was the resemblance of Miss L. to Miss M.

In all these cases the feeling persistently antedated the association of ideas. However, one might explain this in terms of "subliminal association," or the "subconscious."

Whatever is the prime cause, the feeling in such cases surely comes to consciousness first. Therefore, as far as one's knowing about it is concerned one's recognition is primarily a feeling.

From this study the following conclusions are derived.

1. The recognition efficiency is about two and one half

times that of recall, and this ratio varies slightly with different amounts of stimuli and with different intervals of time. However, the total number of words correctly scored in recognition was about four times the number recalled. The difference in ratios is due to the penalty assigned because of chance.

2. Great individual difference obtains for both recognition and recall, but for recognition it was proportionally higher than for recall, and difference of time intervals and length of stimuli-list affect the variability of recall more than that of recognition.

3. The correlation between recall and recognition is surprisingly low: many who recall only one or two words or even none have a remarkably high record for recognition.

4. There is a general increase of incorrect answers and a decrease of correct answers with an increase of time-interval.

5. For long intervals of time more of the answers for recognition are correct than those for recall and this superiority of recognition-answers increases with increase of time interval.

6. Many of the incorrect words given show a high degree of association in form or in meaning with the stimuli-words; general ideas are carried over most frequently.

7. The comparative order of frequency for each word, in recognition and recall is about the same for the first words of the stimuli-list, but there is a wide variation for those least frequently recalled.

8. A knowledge of the number of words to be recognized seems to be a great aid in recognition.

9. The affective element is very much more pronounced in recognition than in recall.

10. The wide difference in results obtained from the different groups under different conditions show how unreliable it is to derive general laws from small groups of subjects arbitrarily tested. Therefore the conclusions herein derived are necessarily limited to the tests described in this study, until they shall have been verified by further studies.

11. Some sex differences are obvious:

(*a*) The girls are superior to the boys for both recognition and recall, and much more for recall than for recognition. Their superiority for recall invariably holds true even when divided into grades, while for recognition this superiority obtains only for large groups.

(*b*) The most striking sex difference is shown in the ratio of recognition to recall; while on the average, the boys' efficiency for recognition is three times that of recall, the ratio by the girls is about two. Likewise the total gain in per cent. in recognition efficiency over that of recall is markedly superior for the boys.

(*c*) A higher correlation between recall and recognition obtains for girls than for boys.

(*d*) For recall the girls seem to be more variable than boys while for recognition the opposite holds true.

(*e*) The girls recall more incorrect words than the boys, while the boys recognize more incorrect words than the girls.

(*f*) Both for recall and recognition a higher percentage of the total answers given by the girls than those given by the boys are correct.

TABLE I

TOTAL AVERAGES

No. Subjects	Recall			Recognition					Ratio of Recognition Efficiency to Recall	Gain of Average Recognition Efficiency Over Recall in Per Cent.	Gain of Total Recognition Over Recall in Words
				Efficiency		Total Correct					
	Words	Per Cent.		Per Cent.		Words	Per Cent.				
		Ave.	A. D.	Ave.	A. D.		Ave.	A. D.			
Bridgeport and Conshohocken (time 1 day)											
M. 124	4.5	22.9	8.9	66.5	14.6	16.5	82.5	10.7	2.90	43.6	12.0
F. 110	5.9	29.4	9.9	70.5	14.5	17.3	86.6	8.4	2.39	41.1	11.3
Mt. Union (time 1 week)											
M. 50	2.3	11.5	6.5	37.2	14.2	12.9	64.3	19.7	3.23	25.7	10.6
F. 64	3.5	17.5	5.9	39.9	13.2	13.6	68.1	16.5	2.28	22.4	10.9
Tyrone (time 10 mo.)											
M. 50	.9	8.6	6.9	29.9	15.2	6.9	68.6	10.2	3.48	21.3	6.0
F. 55	1.5	15.0	12.1	28.7	14.9	6.9	68.9	10.5	1.91	13.7	5.4
Royersford (3 weeks)											
M. 109	1.5	25.5	17.3	60.7	23.9	4.5	75.1	18.7	2.38	35.2	3.1
F. 125	2.0	33.9	19.2	63.0	19.2	4.6	76.0	15.9	1.86	29.1	2.5

TABLE III

TABLE OF FREQUENCY

No.	Correct												Incorrect											
	Recall						Recognition						Recall						Recognition					
	B. and C.		Mt. Union		Tyrone		B. and C.		Mt. Union		Tyrone		B. and C.		Mt. Union		Tyrone		B. and C.		Mt. Union		Tyrone	
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
....	124	110	50	64	50	55	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....
20	....	....	....	....	....	....	11	14	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....
19	....	....	....	....	....	....	23	25	2	4	....	....	....	....	....	....	....	....	....	....	....	....	....	....
18	....	....	....	....	....	....	19	20	1	6	....	....	....	....	....	....	....	....	....	....	....	....	....	....
17	....	....	....	....	....	....	16	17	6	11	....	....	....	....	....	....	....	....	....	....	....	....	....	....
16	....	....	....	....	....	....	21	14	6	2	....	....	....	....	....	....	....	....	....	....	....	....	....	....
15	....	....	....	....	....	....	9	8	5	5	....	....	....	....	....	....	....	....	....	....	....	....	....	....
14	....	....	....	....	....	....	10	5	6	11	....	....	....	....	....	....	....	....	....	....	....	....	....	....
13	....	....	....	....	....	....	5	5	2	6	....	....	....	....	....	....	....	....	....	....	1	1	....	....
12	2	3	....	....	....	....	2	1	3	4	....	....	....	....	....	....	....	....	....	....	....	....	....	....
11	1	4	....	....	....	....	3	....	6	3	....	....	....	....	....	....	....	....	....	....	....	2	....	....
10	1	4	....	....	....	....	3	1	3	5	....	....	....	....	....	....	....	....	....	....	2	....	....	....
9	3	3	....	....	....	....	....	....	2	2	5	8	....	....	....	....	....	....	1	....	2	2	....	....
8	4	10	....	....	....	....	1	....	2	1	12	10	....	....	....	....	....	....	1	....	4	2	....	....
7	9	15	2	3	....	....	....	....	3	....	13	20	....	....	....	1	....	1	....	1	....	5	....	....
6	15	21	....	3	....	....	....	....	2	....	13	11	....	....	2	1	1	....	3	1	6	6	....	....
5	18	18	1	7	....	1	....	....	1	1	5	3	1	....	2	3	3	....	5	4	6	6	....	....
4	29	14	6	17	....	4	1	....	....	....	2	3	....	....	4	3	2	7	14	12	9	7	4	....
3	19	11	12	19	....	10	....	....	....	....	....	....	8	3	2	8	4	6	17	19	4	8	13	9
2	15	4	7	11	13	12	....	....	....	....	....	....	4	4	7	7	10	13	27	21	6	9	13	22
1	5	2	20	3	17	10	....	....	....	....	....	....	21	18	12	22	11	17	29	32	8	6	14	15
0	3	1	2	1	20	18	....	....	....	3	....	....	90	85	21	19	19	11	27	20	2	10	6	9
Average..	4.5	5.9	2.3	3.5	.9	1.5	16.5	17.3	12.9	13.6	6.9	6.9	.5	.3	1.4	1.7	1.4	1.7	2.0	1.9	5.2	3.8	2.9	2.6

Table IV

Per Cent. Correct of All Answers Given

	Recall	Recognition
	B. & C.	
M. 124	90.5	89.1
F. 110	94.8	90.0
	Mt. Union	
M. 50	61.7	74.7
F. 64	69.1	78.0
	Tyrone	
M. 50	37.4	70.3
F. 64	46.9	72.9
	Royersford	
M. 109	75.2	81.0
F. 125	76.3	86.9

THE AUTOMATIC WRITING OF CHILDREN FROM TWO TO SIX YEARS, INDICATIVE OF ORGANIC DERIVATION OF WRITING IN GENERAL

BY ANNA WYCZOLKOWSKA

It was on a steamer going from New York to Hamburg, in the year 1908, that my attention was called for the first time to spontaneous writing by children. I noticed a little girl, three years, and her brother two years of age, scribbling

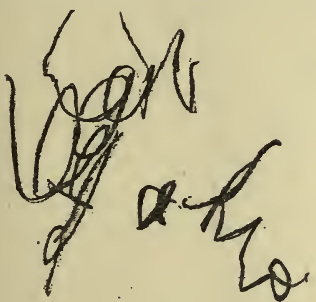


FIG. 1

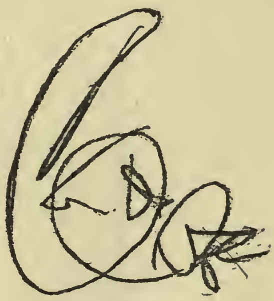


FIG. 2

various lines on a piece of paper. Having become acquianted with the little company, I could enter into possession of some of these graphical specimens reproduced in Figs. 1, 2 and 3.

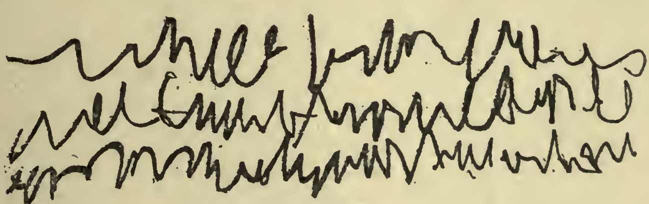
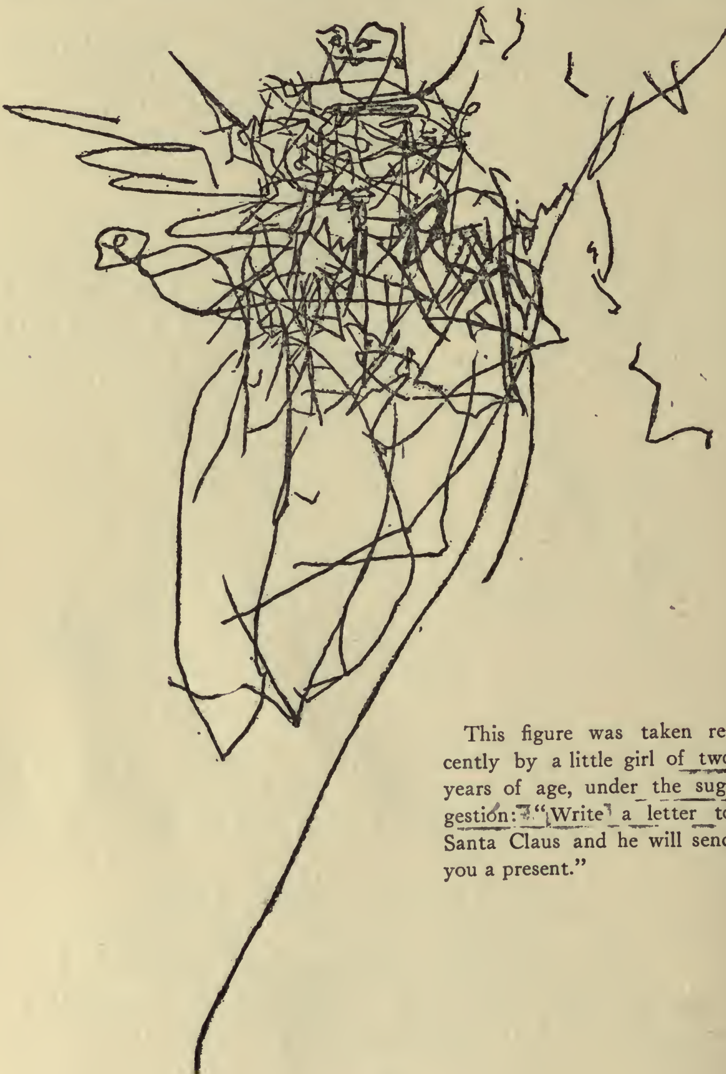


FIG. 3

We see here two different results of automatic movements of the hand projected on paper. Figs. 1 and 2 show delicate and incoherent lines produced by a child two years old, which

starts such exhibit of points and lines with an obvious hesitation and timidity; Fig. 3 on the contrary represents a



This figure was taken recently by a little girl of two years of age, under the suggestion: "Write a letter to Santa Claus and he will send you a present."

Fig. 4

graphical complication unexpected from a child three years of age.

When I returned to Chicago I started a systematic study

of automatic writing by children with a little girl two and half years of age, which observations have been made through two consecutive years, after which time the child began to imitate printed or written letters of adults; consequently was passing from automatic to assimilated writing.

Aldona, who was previously the subject of my observations in reference to speech, had shown during all this time such an inclination to spontaneous writing, that no table or wall, no chair or book was spared from her graphic exhibit, moreover I had a continual insight into the gradual development of her graphic dispositions. When these tests with Aldona were commenced she had already begun to scribble by the projection of mechanic movements of the hand from the right to the left side, limited only by the edges of the paper or blackboard. This probably is the reason why I never could observe in her the earliest graphical signs of children, reproduced in Figs. 1, 2 and 4. Observing her scribbling, produced with much energy and continuous impulses, I noticed that her attention was mostly stimulated by the pleasure in moving the hand and not by the result obtained on the paper.

After a few weeks I could observe a certain change or rather a progress in the graphic capacity of Aldona, when she produced more attentively some concentric, horizontal and vertical lines. The latter especially were characteristic on account of the waving form.—When Aldona was three years and three months old, her mother promised her to arrange a party for her little friends. I took the opportunity to suggest to the child that she write personal letters of invitation. She understood the point and when I handed her a sheet of paper and a pencil, she accepted them and, after short hesitation, asked me: "What am I to write?" "Invite them to the party and say that you will have a good time together" was my answer. Upon this suggestion her hand begun to move quickly across the paper, interrupted only by its edges, and in a few minutes I obtained a new specimen of her spontaneous writing, Fig. 5. The characteristic difference of this writing with that of the previous stage

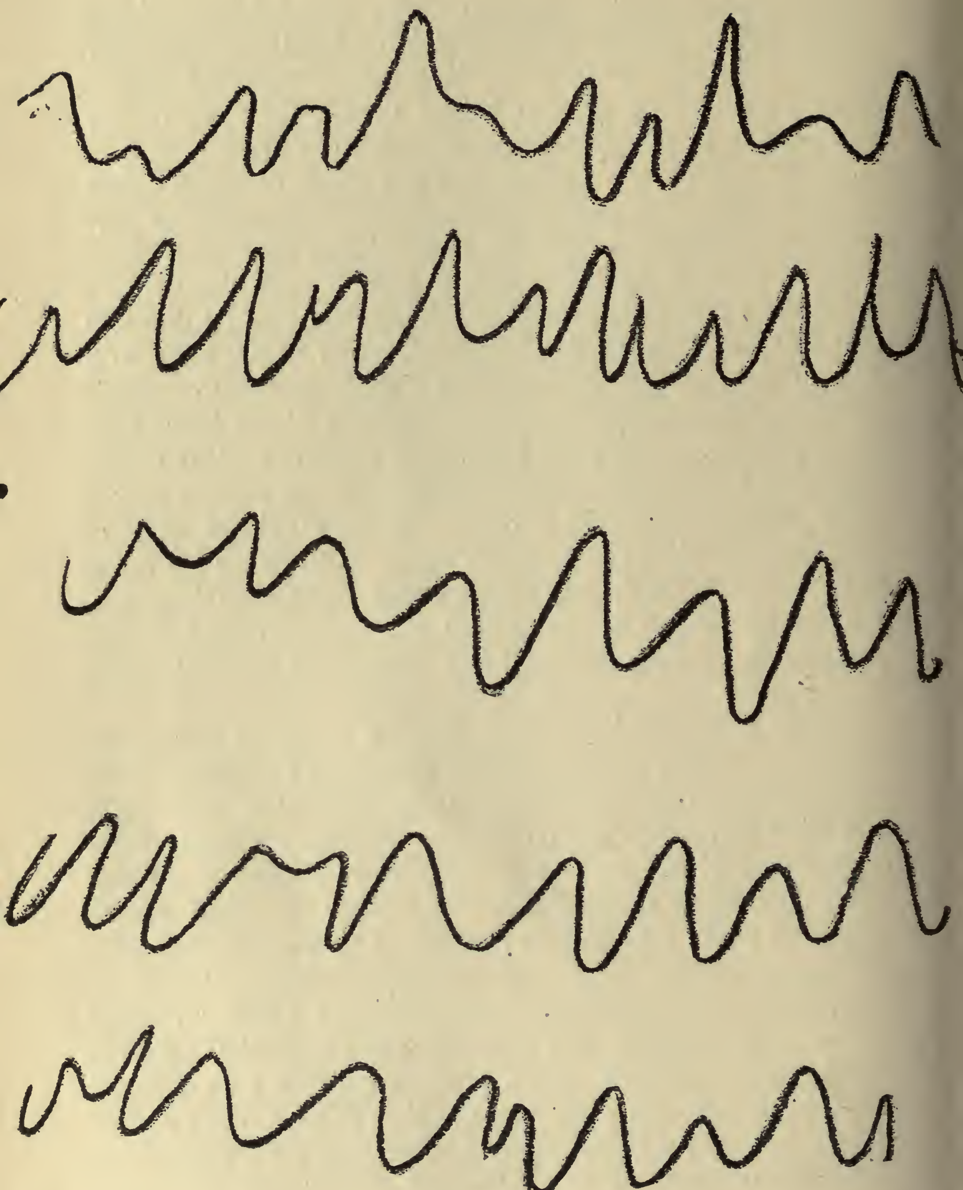


Fig. 5

consists of a considerable diminution of the waving line and the change of relations between the amplitude and the phases of the curve: the first are now higher, the second much shorter. Secondly, I observed a progress in the intensity of attention by this kind of writing. Having finished the letter the child grasps the envelope ready to write the address. In the meantime I remembered two letters received previously, by the mother of Aldona from her niece Dorothy,

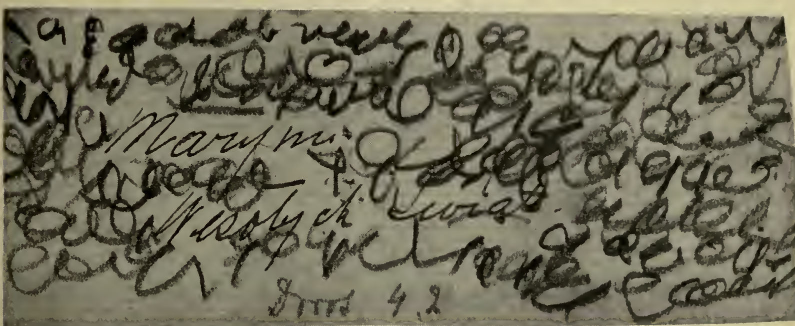


FIG. 6

two years older than her own daughter. These two letters reproduced in Figs. 6 and 7, as the most interesting specimens of spontaneous writing by children, show no affinity to the previous letters of Aldona. Both letters of Dorothy are of a more artificial structure, and the second is especially characterized by an important phenomenon, namely, that many letters of the alphabet of different languages like German, Greek, Latin and even Arab or Sanscrit can be easily detected in this stage of automatic writing evolution, which detail will be explained later.

The question became more interesting but at the same time more difficult to explain, because these two kinds of letters were not only different in their character but even were contrasting each other, the first being uniform and automatic, the second complicated and artificial.

A new stage in the graphical development of Aldona has thrown a new light concerning the multiplicity of this phenomenon. When Aldona was four years old she abandoned

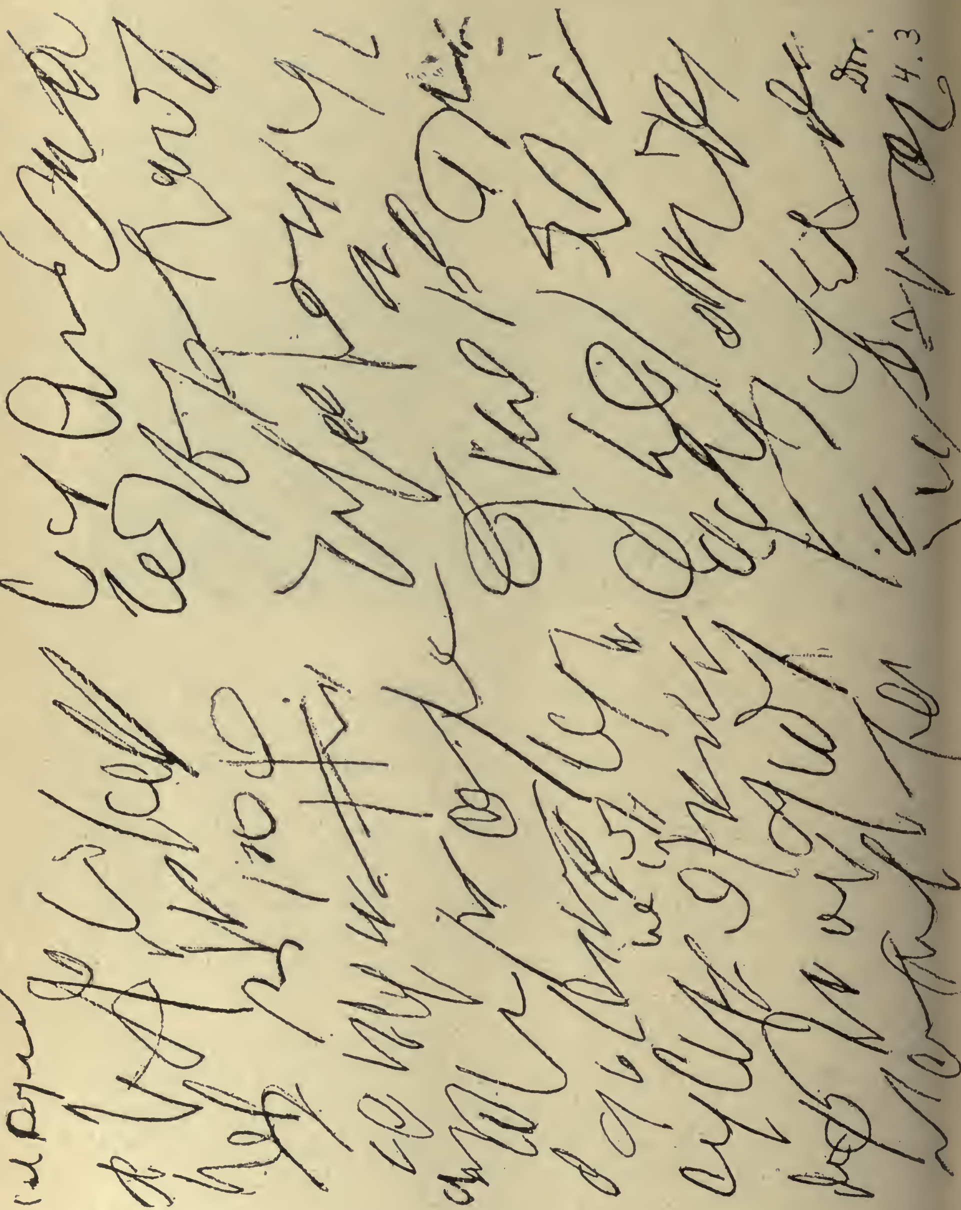


FIG. 7

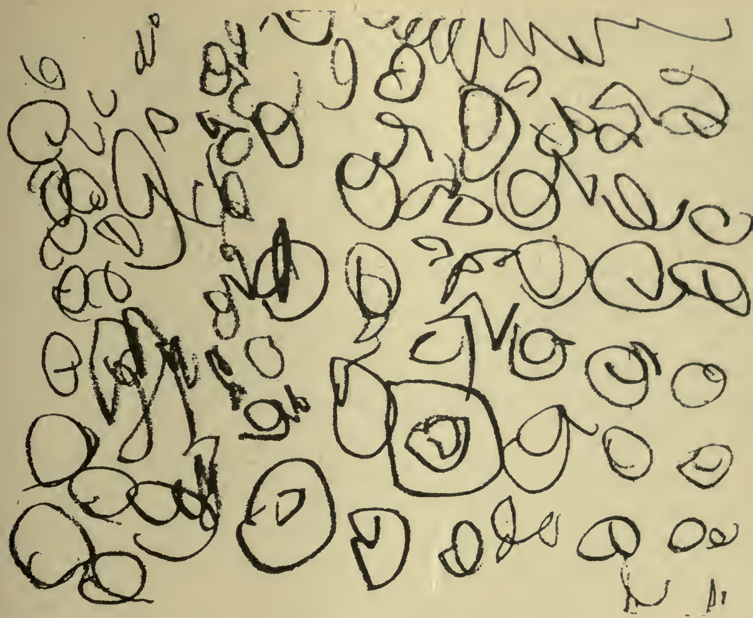


FIG. 8

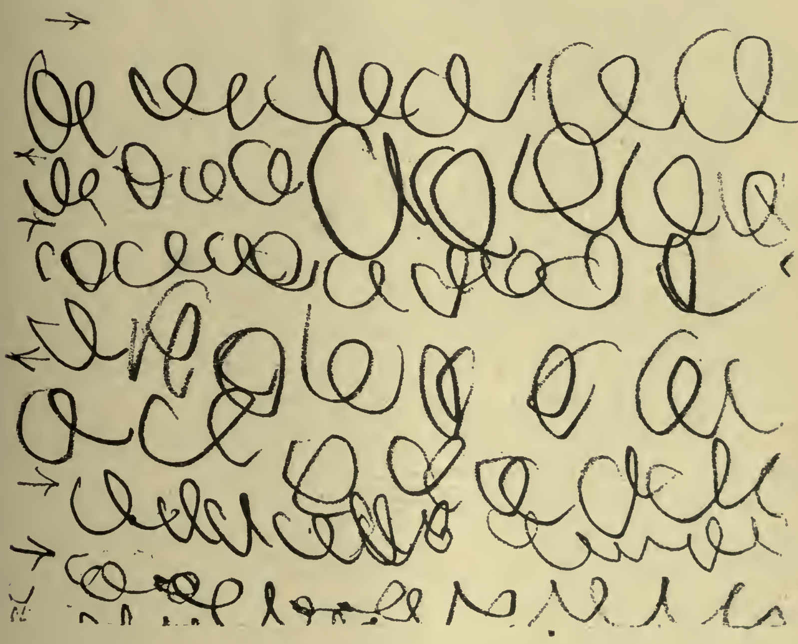


FIG. 9

her recent habit of writing continuous curves, changing it to a kind of combined zigzags, more or less connected with each other (Fig. 8). If we compare this new specimen of her writing, as well as the other specimen *very similar*, taken recently by a little girl, Alicia, four years and eight months old (Fig. 9), with the first letter of Dorothy (Fig. 6), we become aware of the affinity between all of them. They differ only so far in this respect that the writing of Aldona, Fig. 8, and Alicia, Fig. 9, can be easily analyzed in their isolated zig-zags, while the same method can hardly be adapted (without the use of a magnifying glass) to the letter of Dorothy with its density of lines, and yet the affinity between all of them can not be denied.

This incident and this comparison explain the important fact that the automatic writing of children is susceptible of an evolution, consequently every stage of the latter depends upon the corresponding age of the child between two and six years.

A new stage of graphical evolution by Aldona was apparent when she continued to produce absolutely isolated and disparate graphic specimens, which like the second lètter of Dorothy show many affinities with Latin and Greek, Figs. 10 and 11 and even with oriental symbols. We perceive often on these kinds of letters W, M, N, O, B, S, frequently e and t, seldom or never k, g or d, because of their complexity of lines. Besides we find letters and symbols like the following Fig. 12.

The last stage of this evolution was a mixture of automatically produced lines and their complexes and the beginning of imitation of letters of the alphabet printed and written by adults. Aldona was then four years and eight months of age but very premature in her mental evolution.

We have thus detected five following distinct stages in the evolution of automatic writing by children.

1. (*a*) Incoherent lines produced with obvious timidity and clumsiness in moving the hand (Figs. 1, 2 and 4).

(*b*) Automatic and unattentive scribbling or chaos of straight and concentric lines, limited only by the edges of the paper, by two to two and a half years old children.

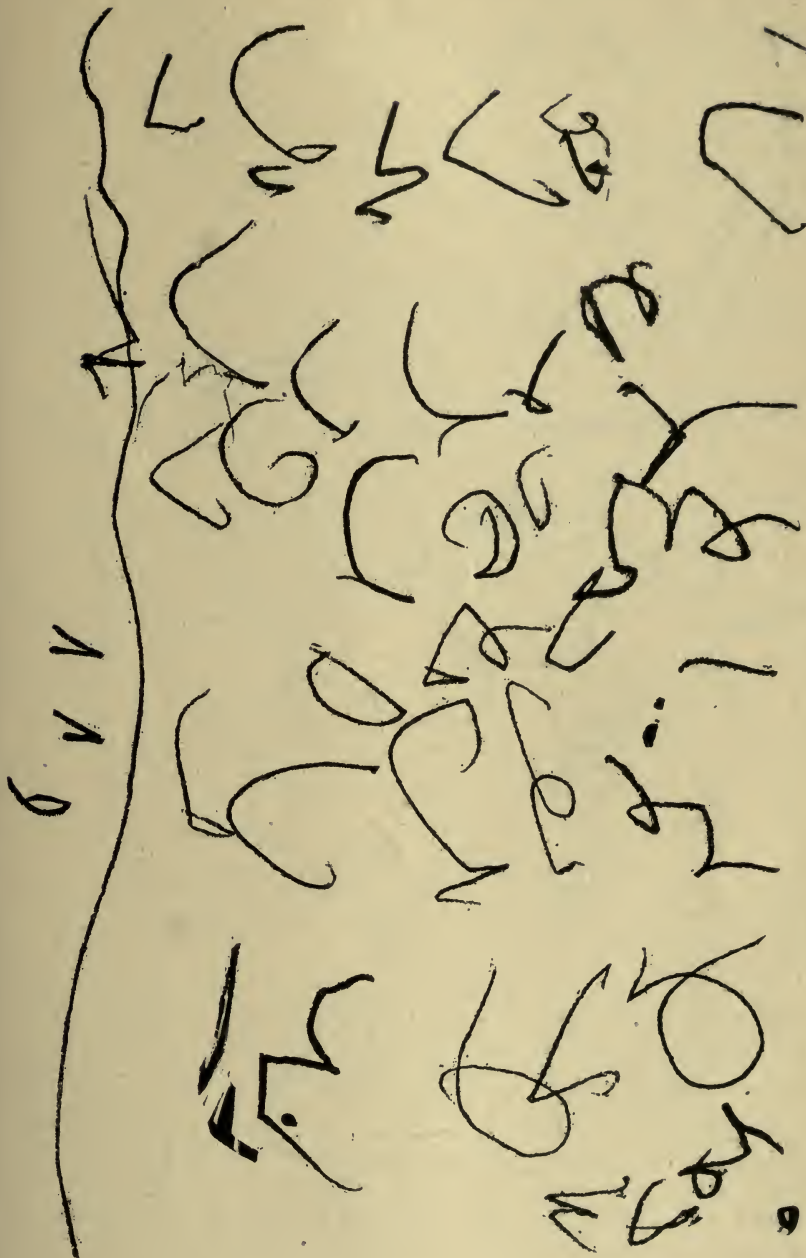


Fig. 10

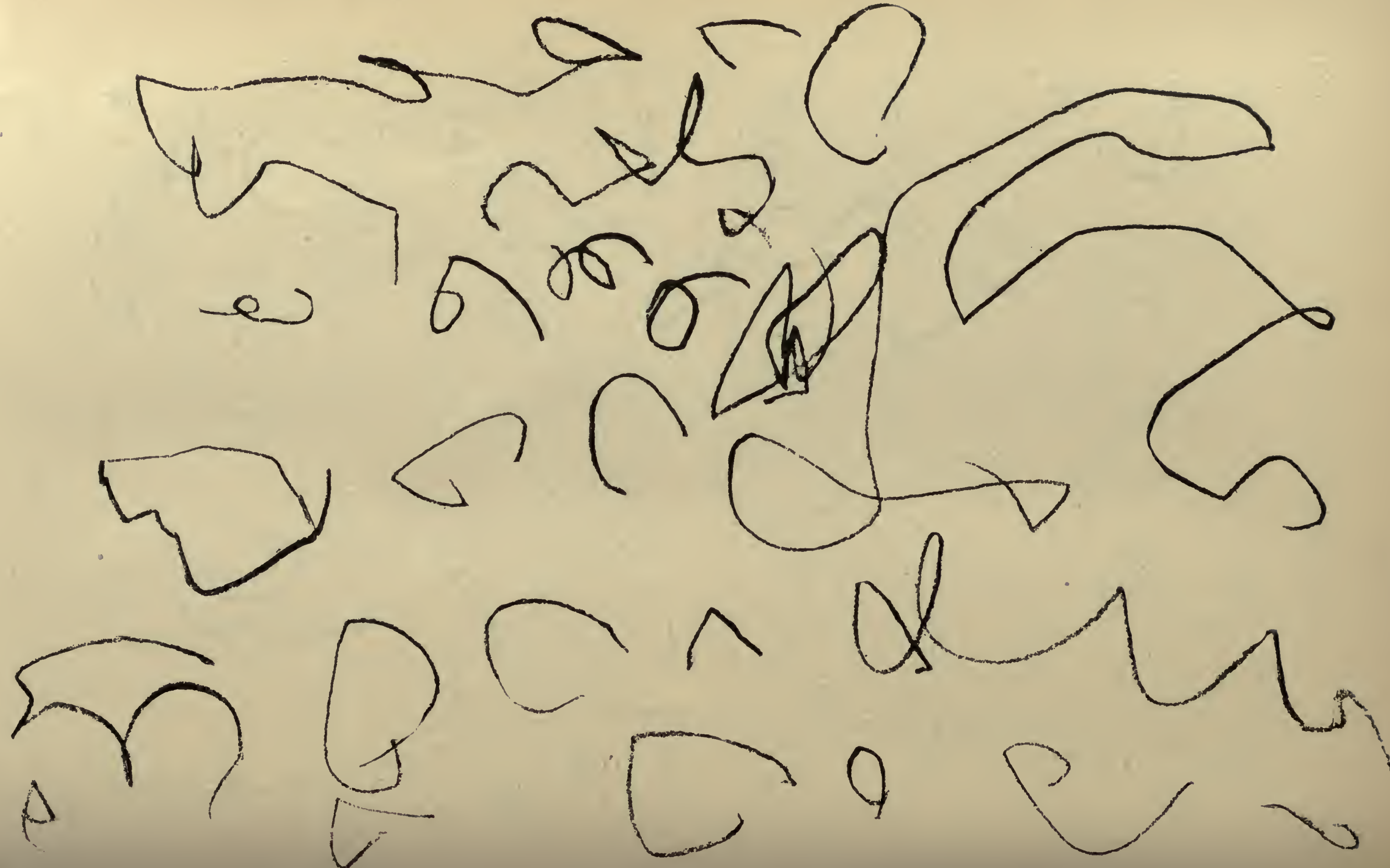


Fig. 11

2. Circular, perpendicular and horizontal waving lines with small amplitude but very long phases in two and a half to three years old children.

3. Continuous curves with high amplitudes and a notable diminution of phases, in three to five years old children with much attention brought into the writing exhibit (Figs. 5 and 13).

4. More or less isolated zig-zags with unconscious imitation of letters and symbols of the writing in various languages (Figs. 6, 8, 9 and 7, 10, 11).

5. Conscious imitation of printed or cursive writing of adults, mixed with the previous graphic elucubrations.

II

One question remains now open and that is: can these results of evolution of automatic writing observed in a few children only, be expected in every individual between the age of two and six years?

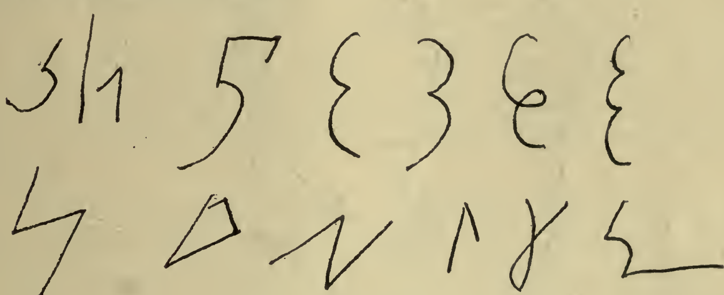


FIG. 12

In order to answer this question I submitted to special tests, shortly before the publication of this paper, many children of friends and especially children in a kindergarten school, when I was able to demonstrate all the five stages of spontaneous writing previously described (Figs. 4, 9 and 13). No child was opposed to such a test, although it sometimes happened that a child of retarded mental evolution was also retarded in its graphico-automatic development; but it never happened that a normal child three years of age should produce a writing of the first stage, or that a child two years old

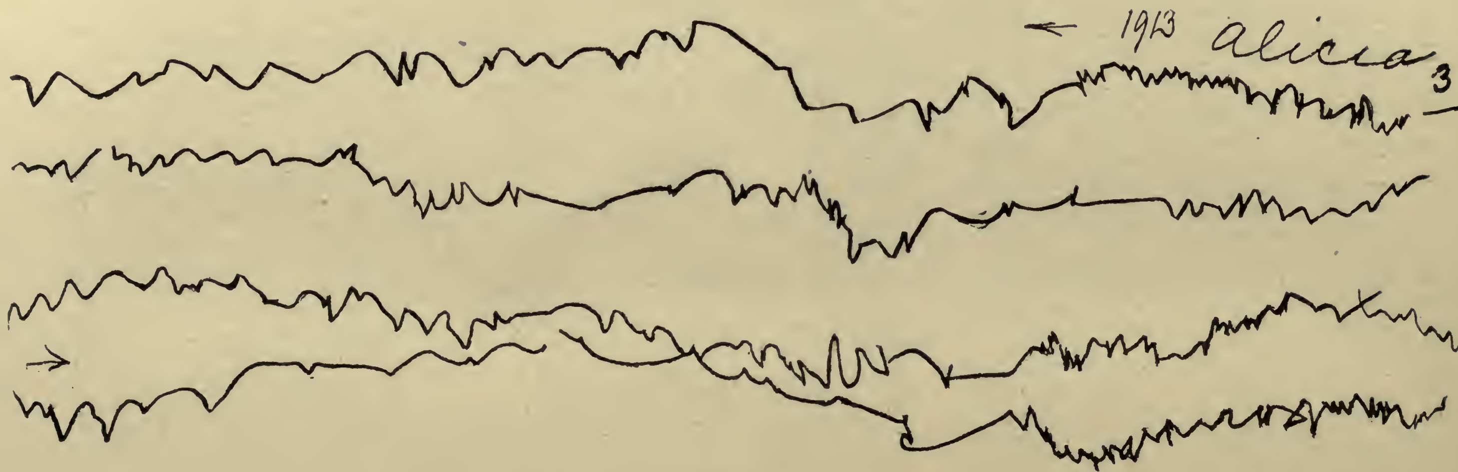


Fig. 13

should show the capacity of an older one. The smaller children will always start with incoherent lines, betraying timidity and lack of adroitness of hand movements, while a child of three to four years will display a complexity of writing, characteristic of special affinity to the writing of adults in various languages.

At the same time I was able to make some new observations. Thus, for instance, it is easy to guess the age of the child from a specimen of its writing, as it is easy to judge of its state of general development from the evolution of spontaneous writing, whether retarded or premature. When on one occasion I showed my surprise to the teacher of the kindergarten school that a girl of four years of age could only produce straight, horizontal and vertical lines, the same teacher, not aware of the above result of my investigations, informed me that the child seemed abnormal in everything else. Another observation in reference to spontaneous writing suggested to me by two little girls of the kindergarten school is that some children write spontaneously from the right side of the paper to the left using (Figs. 9 and 13) however their right hand. On my special request one of them began to write both ways and it became obvious that her writing from left to right was much slower than in the opposite direction.

In reference to psychological considerations, I observed that children from two to two and a half years of age are writing without attention, displaying simply automatic movements of the hand for the pleasure of moving; children from three to four years write with more interest and confidence of their graphical ability, while those of five to six years are more sceptical because they have some perception of the existence of the cultural writing of adults. But when requested to write whether alone or in company, they show the will to do their best and without any tendency to conscious imitation of each other. I let seven children write at the same time at one table, and I obtained specimens of different stages of evolution in spontaneous writing each corresponding to the age of the child. This opportunity to observe many children in

reference to spontaneous writing has confirmed the previous statement, that every stage of graphical evolution is connected with corresponding age in accordance to the degree of general development of the child. We have therefore the right to claim with all certainty that every child without exception is subject to a graphico-automatic evolution, which can be strictly described, and which in a certain degree helps it to the acquisition of the trained writing.

Extending our views on writing in general we can contend with equal right that graphical faculty must have been in the dim past the direct cause and source of the impulse which had for its aim the beginning of writing in general.

III

It was mentioned before that it is possible without any difficulty to detect in the third and fourth stage of automatic writing by children some letters of the alphabet of various languages, modern and ancient, which fact requires explanation. This phenomenon can not be regarded as an effect of imitation but as a result of the existence of the same graphical components in both the automatic and trained writing.

Thus if we analyze all the stages in the evolution of automatic writing of children, we find in it four or five graphical elements like the point, the horizontal and vertical, the circular or semi-circular line and especially the waving line in which the relation between the amplitude and the phase continuously changes.

Passing now to a similar analysis of cultural or trained writing, we have first to take account of two different modes in their historical evolution, from which one operate especially with disparate symbols like in oriental languages, and the second introduces the curve connecting different letters of the alphabet into words, like in modern languages. In the first category of cultural writing the graphical elements are about the same as in the automatic writing of children, namely the point or groups of them, the vertical and horizontal, circular and semicircular line; in the second group the same graphical elements are used as before, but combined and

joined with a continuous curve. The latter plays an important part thus in both the cursive and the automatic writing of children.

These complexes of lines used in cursive writing are the following: the circular line, like in "o"; the union of the latter with a part of the waving line, like in "a" and "d" or "g"; the combination of the vertical and semicircular line like in "blpcmnrpsuw" etc.; the compounds of the vertical and horizontal like in "t"; and at last the crossing of two lines, like in x and z.

We must concede now that in every language some elements or their complex prevail. Thus the circle or semicircle with a point or a group of points has the preponderance in the Arabic writing. Of 27 letters 21 are circular. In the Hebrew the combination of a vertical and horizontal line is prevalent. In 23 Hebrew letters 19 are of this kind. Sans-

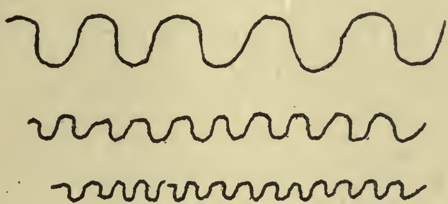


Fig. 14

crit shows a more complex structure and especially a combination of both of the former elements, prevailing in Arabic and in Hebrew. The crossing of two lines appears nearly in all of these languages although having different phonetic meaning. In Latin prevail the round, in German the vertical line and so forth.

In consideration of the fact that the continuous curve, or waving line is the most important element in both, the automatic writing of children and in writing of modern languages, we will conclude this study by referring to a detail of small importance, which however seems to be of significance for the demonstration of the origin of writing.

During my long observations of spontaneous writing by children I noticed incidentally that it is impossible for a child

as well as an adult to design the following continuous curve (Fig. 14) on a small scale without successive interruptions, which result from organic impulses. These interruptions in the curve decrease as the curve increases in size, finally disappearing; they become more and more apparent as the curve diminishes; these are like two limits between which the hand movements accommodate itself to writing.

But the important point of this view is that the above continuous curve like the various stages of automatic writing of children as well as their elements cited above, seemed to confirm the theory of organic origin of cursive as well as of all other writing. We are therefore inclined to believe, that as it is necessary, as I think, to recognize in the phonetical elements of a child speech the organic means from which human speech in general evolved, so it is equally essential to recognize in the graphical elements of the automatic writing of children with all its stages and especially in ability to produce the continuous curve, the organic basis from which the cultural writing has evolved.

VARIATIONS IN EFFICIENCY DURING THE WORKING DAY

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In the general literature of this topic there is a rather common failure to distinguish carefully between two questions which are really quite different from each other. There is on the one hand the question explicitly formulated by Marsh in his study of 'The Diurnal Course of Efficiency.'[1] Are there normally recurring periods of efficiency or inefficiency during the working day? Such variations, if they exist, might be due to such factors as changes in temperature, humidity, electrical conditions, meals, waking and sleeping, etc., extra-nervous changes, that is, which might supposedly be present independently of the character or degree of the activity of the nervous system. Or they might be due to essential nervous rhythms. Such influences it would be exceedingly difficult to disentangle from the many other factors bearing on momentary efficiency in a work process,—such factors as presence or absence of previous work during the day, character and amount of such work, etc. With such influences the present study is not concerned. There is, indeed, no evidence that they exist.

The other question has to do with the ability to do work continuously through long periods of time, the influence of work already done on momentary efficiency and on work yet to be accomplished. Here belong the many studies of fatigue, such as those of Cattell, Thorndike, Arai, and others.

Two things detract from the conclusiveness of most of these studies, however much the investigators may have endeavored to avoid them. The first has been the failure to reach practice limits in the work measured before beginning

[1] *Archives of Psychology*, Columbia University, No. 7.

the fatigue experiments. As a result of this failure most findings of no loss of efficiency with continued work mean merely the balance of loss and gain from two distinct sources. Findings of fatigue, similarly, have meant only a surplus of loss over gain.

The second defect has been the failure to extend the work period over a long stretch of time, or, when long periods were considered, the failure to secure intermediate measurements between beginning and end of the periods. The result of the first form of this defect is that we have but few accurate pictures of the course of efficiency through a whole working day. That of the second form is that the course is, in these studies, not shown at all.

A fortunate opportunity to study the work of a fairly large number of individuals (16) for a considerable number of working days (40) with the full time of the subjects at the disposal of the experimenter and with all the working conditions, such as daily routine, incentive, practice, etc. under unusually full control, was so planned as to afford data on this important phase of the psychology of work. The chief results bearing on this topic are here presented.

The Present Experiments

During the course of a prolonged investigation of the influence of caffein on performance in simple mental and motor tests, the schedule of doses and assignment of squads were so arranged that a study of normal performance on control days could also be made. One group of four people constituted a control squad throughout the experiment. The remaining eleven subjects were given the caffein doses according to a schedule which gave 17 coincident control days for all of the individuals. Including the corresponding days from the records of the control squad, this gave 17 normal days for each of 15 subjects. In the long run these days alternated with drug days, after the first week. The records for this week and for two preceding trials are discarded in the present study. They served to bring all individuals down to the secondary slope of the practice curve, and much to

reduce the variability of performance. This leaves 10 days for each of the 15 subjects. On two further days intensive tests were made on 10 subjects, working on an almost completed practice level.

The tests and technique employed have been fully described in an earlier monograph,[1] and no further account of them need be given here. Briefly enumerated, they are as follows:

1. *Tapping*—400 taps, maximal speed, elbow rest, stylus.
2. *Coördination*—100 strokes on the "three-hole" target.
3. *Color Naming*—Columbia color-naming chart, 100 squares.
4. *Naming Opposites*—50 words, medium difficulty, chance order.
5. *Calculation*—adding 17 mentally, 50 two-place numbers, chance order.
6. *Steadiness*—holding at arm's length, for one minute, a brass rod 2.5 mm. in diameter, in a hole 4.5 mm. in diameter, with as few contacts as possible.
7. *Discrimination Reaction*—reacting with right hand to red and with left hand to blue, time measured in sigma.

Such conditions of work as incentive, exercise, rest, temperature of the room, personality of the operator, etc., were fairly constant throughout the experiment, and during the latter part, on the two intensive days, conditions were still more rigorously controlled. All individuals ate at the same table, spent the whole day under the eyes of the experimenter, etc. The quality and quantity of the work, in each test, remained constant, the records being made in time of performance. The subjects were college students, graduate students, and the wives of some of the latter. All received compensation for their work, were under oath to do their best at each trial, were zealous to an unanticipated degree, and were stimulated to further rivalry by the award of prizes for 1st and 2nd places in each test. No records were disclosed during the experiment, except that after each week the names of the best three persons in each test were posted.

[1] 'The Influence of Caffein,' *Arch. of Psychol.*, Columbia Contributions, 1912, 166 pp.

To secure greater interest and effort for experimental purposes than was displayed by these individuals seems to the writer to be well nigh impossible. These individuals were not "experimenting" in the usual laboratory sense. They were regular employees, working for wage, at real work, and outside candidates were waiting to take the places of any whose work seemed unsatisfactory. All in all the experiment constituted a fairly accurate duplication of the conditions of actual work. The chief differences were that in this case the work was standardized, the conditions were controlled, and the workers were directly and intelligently interested in the method and outcome of their work.

During the preliminary part of the experiment five trials were made daily, each sitting requiring about 45 minutes. The test hours were 7.45 A.M., 10:00 A.M., 12:15 P.M., 3:10 P.M., and 5:30 P.M. During the second part of the experiment the subjects arrived at the laboratory at 10:00 A.M. and worked continuously for about 12 hours, except for two 45-minute periods for lunch and dinner. One or two of the tests were omitted in this section, and only 10 individuals gave records which could be used for the present purpose. Two control days during this section gave 15 trials each in all of the tests. This gives then 20 records (two trials for each of 10 individuals) for each test hour, the working day extending from 10:00 A.M. to 10:00 P.M.

Another individual did not take part in these tests, but worked under the same conditions and during the same periods, at typewriting. The results from this subject are discussed in a separate section of this report.

Preliminary Experiments

In this experiment there were 10 experimental control days for each of 15 subjects. These subjects are here separated into three different groups. Group I. consists of the five women; Group II. of five men whose attendance was perfect throughout both the preliminary and the intensive experiment; and Group III. of five men whose records have been grouped together because of irregularities of one kind

or another during the investigation as a whole. One subject was ill part of the time, another was compelled to leave the city on one or more days, another did not participate in the intensive experiment, another was dismissed because of

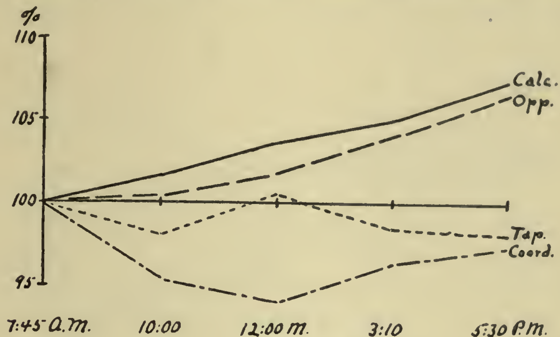

FIG. 1. Group 1, five women.

failure to comply with all the instructions while in the laboratory, and the fifth was regular but was included in this group so as to give five subjects in each of the three groups.

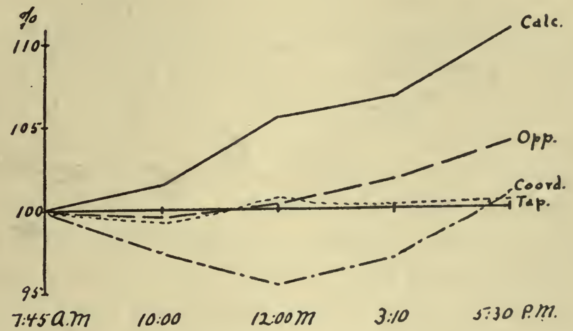

FIG. 2. Group 2, five men.

Since the members of the groups were not chosen because of their records in the tests but on these purely incidental grounds, the performance of one group will well serve to check up the performance of other groups. As a matter of fact, all three groups show the same results. The individual records are not given in this preliminary experiment, because more detailed records and probable errors are given in full

for the intensive section. The preliminary results are given in the form of curves for each of four tests and for each of the three groups. Each point in the curve is the average of 50 separate determinations, 10 records by each of 5 subjects. This gives high reliability to the course of the curves.

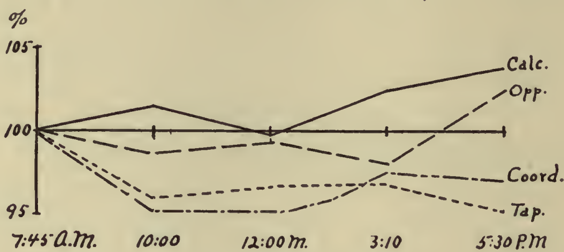

Fig. 3. Group 3, five men.

The initial performance (at the 7:30 A.M. period) is taken as the unit of measurement. Each separate record for each subject is then expressed in terms of per cent. of the morning record. This makes all records roughly comparable in spite of the differences in absolute time of performance in different tests or by different individuals.

Plate I. gives the results for the group of five women. *Calculation* and *Opposites* both show increasing fatigue at successive trials. In *Calculation* the decrease in efficiency is regular, being about 2 per cent. at each trial, giving 7.5 per cent. fatigue at the end of the day. In *Opposites* the second trial shows little inferiority, the third trial shows a falling off of 1.5 per cent., and the 4th and 5th trials each give an additional decrease of 2.5 per cent. below the preceding trial, the total thus being 6.5 per cent. by the end of the day. The curve for *Tapping* is more irregular, but its general tendency indicates increased speed rather than slowness. Only the mid-day record is slower than the initial time (.5 per cent.), all other records are 1.5 to 2.0 per cent. faster than the initial time, and the fastest record is made at the end of the day. The *Coördination* test shows increasing efficiency up to mid-day, then a falling off from this maximum, but the falling off, by the end of the day, is several per cent. less than

the previous gain. All subsequent records are better than the initial trial, the maximum efficiency coming in the middle of the day. The gain by mid-day is some 6.0 per cent. over the initial time.

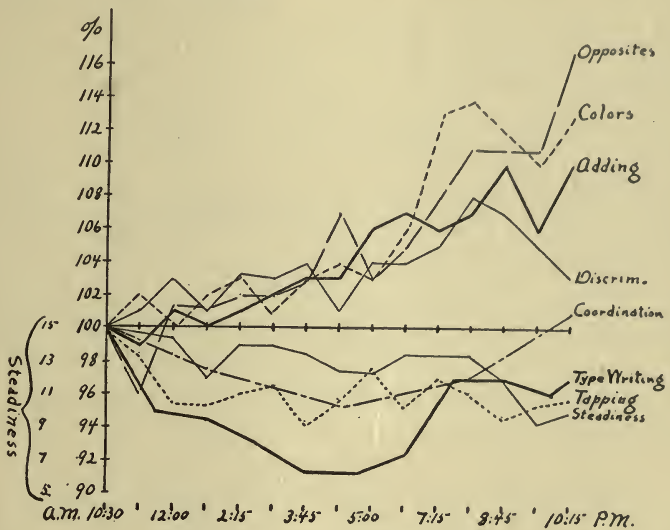

PLATE IV. Intensive Experiment.

Showing the effect of continued work on the different tests. Each record is the average of 20 determinations, 10 observers, 2 days each. Records are given in *per cent.* of initial performance, except in the case of *steadiness*, where the number of contacts is given.

Plate II. gives the records made by Group II. The results are very similar to those yielded by Group I. The fatigue in *Calculation* amounts to 10.5 per cent. by the end of the day, and is regular in its appearance from trial to trial. *Opposites* shows a smaller loss (4.0 per cent.) and most of this loss comes, as was the case with Group I., in the 4th and 5th periods. *Tapping* shows no increased efficiency with this group, but neither is there any fatigue present, the records yielding practically a horizontal line. *Coördination* shows, as with Group I., an increase of several per cent. (4.5 per cent.) by the mid-day period, and a subsequent loss, which, in this case is just equal to the previous gain.

The curves for the irregular group (Plate III.) are, as is to be expected, irregular, but their general tendency quite

agrees with the results from the two other groups. *Calculation* and *Opposites* show inefficiency by the end of the day, and the loss in the former is greater than in the latter. *Tapping* shows improvement rather than loss, and the best record is made at the last trial. The initial trial is by far the slowest of the five records. *Coördination* shows the now familiar increase by mid-day, with subsequent loss; as with Group I., the loss is smaller than the previous gain.

In general then the result of the preliminary experiment is as follows: The two more strictly mental tests show decreasing efficiency as the result of continued work through the day. The more strictly motor tests show increase rather than decrease. In *Tapping* this increase is most noticeable at the end of the day; in *Coördination* the period of maximal efficiency is greatest at mid-day. A closer and more detailed study of these relations requires the intensive experiment, the results of which follow.

The Intensive Experiment

Table I. gives a summary of the results in this section of the investigation. There were two days of work for each of 10 individuals. Each average is thus made on the basis of 20 determinations. The table gives the average time of performance in each test at each of the 15 trials between 10:30 A.M. and 10:30 P.M. The averages are expressed first in terms of absolute time (seconds). In this case the probable error of the divergence of the true from the obtained average is given at each point. By this time the individuals were all working on a practical practice level in all tests, and the probable errors are very small. The averages are also given in terms of the initial trial as unity, as in the preliminary section. The initial record thus becomes 100 per cent. and subsequent records are expressed in per cent. of this initial performance.

Six tests are included in this section. *Color Naming* and *Discrimination-reaction Time* and *Steadiness* are added to the tests used in the preliminary section, and Coördination is omitted.

Table I

Averages for the Two Intensive Days, 15 Trials each Day.

Ten Individuals, Two Days Each

Table gives averages and P.E. (t–o) for all tests*

Time		A. M.		M.	P. M.											
		10:30	11:15	12:00	1:30	2:15	3:00	3:45	4:15	5:00	5:45	7:15	8:00	8:45	9:30	10:15
Tapping	Av....	49.8	49.2	47.8	47.7	48.0	48.3	47.2	47.9	48.9	47.7	48.5	48.1	47.4	47.7	47.9
	P.E...	.9	1.1	1.2	1.3	1.3	1.4	1.3	1.5	1.1	1.3	1.4	1.4	1.5	1.4	1.5
	%....	100	98.4	95.6	95.4	96.0	96.6	94.4	95.8	97.8	95.4	97.0	96.2	94.8	95.4	95.8
Color Naming.	Av....	52.1	52.9	51.8	52.9	53.5	52.7	53.8	53.9	53.8	55.4	59.1	59.3	58.2	57.0	59.0
	P.E...	1.6	1.7	1.6	1.4	1.7	1.8	1.8	1.9	2.0	2.2	2.6	2.3	2.0	2.2	2.1
	%....	100	102	100	102	103	101	103	104	103	106	113	114	112	110	113
Opposites....	Av....	34.3	33.0	34.5	34.7	34.8	35.1	35.5	36.6	35.3	35.9	37.1	38.2	38.1	38.0	40.1
	P.E...	1.2	1.1	1.5	1.2	1.1	1.4	1.4	1.5	1.4	1.2	1.5	1.5	1.6	1.6	1.7
	%....	100	96	101	101	102	102	103	107	103	105	108	111	111	111	117
Adding .	Av....	86.1	84.8	86.9	85.8	86.6	87.9	88.5	88.5	91.1	92.3	91.5	91.8	94.9	91.5	94.5
	P.E...	2.1	2.8	2.5	2.3	2.2	2.5	2.8	2.5	2.3	2.6	2.5	2.8	2.8	2.9	3.0
	%....	100	99	101	100	101	102	103	103	106	107	106	107	110	106	110
Discrimination . .	Av....	328	331	338	331	339	338	342	333	342	343	344	355	351	344	338
	P.E...	6.4	7.2	7.4	7.4	8.4	7.2	7.4	7.4	7.6	8.8	7.2	8.2	8.2	7.4	6.8
	%....	100	101	103	101	103	103	104	101	104	104	105	108	107	105	103
Steadiness	Av....	15.0	15.0	14.6	12.0	14.2	14.2	13.8	12.7	12.5	13.5	19.1	13.6	11.9	9.6	10.1
	P.E...	3.5	2.6	3.6	2.5	3.5	3.9	3.6	4.5	2.6	3.8	3.5	3.0	3.8	2.2	3.0
	%....	100	100	97	80	94	94	92	85	83	90	126	91	79	64	65

$$* \text{P.E. (t–o)} = \frac{.84435 \text{ A.D. dist.}}{\sqrt{n}}.$$

This form of P.E. means that in several such experiments the various divergences of the obtained averages from the true average would be distributed about o as a mode, with the figure given as the P.E. of the distribution in question.

The measures, in terms of per cent. of initial performance, are also expressed graphically in Figure IV. Because of the great range of change in the *Steadiness* test (from 100 per cent. to 64 per cent. at the end of the day) the records for this test are not expressed in per cent., but in number of contacts made.

The difference between the mental tests and the motor tests, already pointed out in the preliminary section, is at once apparent in this intensive experiment. The four mental tests show pronounced decrease in efficiency as the day progresses. The motor tests show just the reverse tendency,—an increase in speed of tapping and an increase in steadiness. The eleventh trial in *Steadiness* shows a large increase in number of contacts, but this was the test just after the individuals had come into the laboratory after the evening meal, with its attendant general activity and excitement. This record is given in the table, but omitted from the curve. The *Steadiness* curve, when thus drawn, shows a clear increase in steadiness by the end of the work period. The curve for *Tapping* shows no such uniform increase, but the first two trials are the slowest of the 15, and the four trials at the end of the day are among the best of the 15.

Performance in the four mental tests becomes less efficient at a fairly uniform rate, up to about the 10th trial, beyond which point the separate curves begin to diverge. *Opposites* and *Color Naming* show, at the end of the day, the greatest amount of loss. *Calculation* shows a somewhat smaller loss than these two tests. *Discrimination* ceases to show loss after the 12th trial, the next three trials showing an increasing efficiency instead. The last trial of the day, in *Discrimination* is thus about equal to the average record at the middle of the day. It is to be noticed that in only two cases, in these mental tests, is the initial record ever surpassed at later trials,—namely at the second trial in *Adding* and *Opposites*. In these tests the initial work seems to have served as a warming up process, which facilitated somewhat the next trial. But beyond the second trial the initial records are never excelled. This is in striking contrast with the motor tests, in which the initial trial was, without exception, the least efficient trial in the whole day's work.

It should be noted that during these intensive days there was practically no improvement in any of the tests, either during the day or from day to day. *Adding* and *Opposites* are the only tests which show any such gain at all, and here

the gain from one day to the next is all that shows and this amounts to only 1 per cent. and 3 per cent.

Typewriting

One individual, a woman of 38 years, already proficient in typewriting by the touch method, worked at typewriting instead of at the tests just discussed. Ruskin's 'Sesame and Lilies' was chosen as material to be copied. Each page contained 27 lines, the lines averaging 35 characters each. The subject kept her own time records, and corrected all mistakes noticed, at the time they were made. During the intensive experiment the subject came to the laboratory daily at 10:00 A.M. and wrote four pages each hour, up to 9:15 P.M. (excepting short intermissions for lunch and dinner). This made 10 trials each day. Two of the three days are used in this connection, one control day and one day on which the caffein amount was such that no drug influence was present. The times, for each hour, in minutes, are as follows.

Table II

Typewriting Records

	Hours of the Day										
	A. M.		P. M.								
	10	11	1	2	3	4	5	6	8	9	
1.........	30.0	29.6	29.6	27.8	28.1	28.3	27.8	29.6	28.8	29.0	Time.
	66	65	60	48	52	45	43	52	34	38	Errors.
2.........	31.0	28.5	28.1	29.0	27.5	27.3	28.5	29.8	30.6	29.6	Time.
	68	36	55	65	34	34	43	53	55	60	Errors.
Total......	61.0	58.1	57.7	56.8	55.6	55.6	56.3	59.4	59.4	58.6	Time.
	134	101	115	113	86	79	86	102	89	98	Errors.

Although the figures are few, they are fairly reliable, since the work period measured at each trial was half an hour of continuous work, thus allowing abundant time for compensation of incidental variations. On both days the time of performance decreases to a point of maximum speed at 3:00 and 4:00 o'clock. Beyond this point speed is reduced until by 9:00 o'clock the same time is required as at the beginning of the day's work. When the results for the two

days are added together, this result is very clearly shown. There is constant increase in speed up to 3:00 and 4:00, then gradual loss again. This increase in speed is not gained at the expense of accuracy. Quite the contrary, the hours of fastest speed are also the hours at which the fewest errors are made. The numbers of errors show the same tendency to reduction at this period of maximum efficiency, with increase on either side. The net result of the experiment on typewriting is then quite like that found in the simpler coördination test, in the preliminary experiment, except that the maximum efficiency comes at a somewhat later point in the former than in the latter. A morning inertness with a mid-day gain and an evening loss which is slightly less than this gain, is the general rule for both tests. The same rule holds for accuracy as for speed.

These coördination tests doubtless stand more or less on the borderline between the mental and motor performances, since they involve a considerable amount of perception, discrimination and control, although depending, in their actual performance, largely on the actual motor speed which is possible. The trough-like curve shown by the coördination tests is apparently a combination of the effects on the two extreme types of process. During the middle of the day the increasing motor speed is sufficient to overcome the mental fatigue which comes fairly slowly for the first few hours. After that point the mental fatigue increases so rapidly as to quite offset the effect of the increased motor speed which is still present.

Discussion

It is interesting to compare these results with those of Cattell, Thorndike, Arai, and others who have made extended practice and fatigue curves extending over long work periods, and with the results of Marsh and others who have sought for evidence of normally recurring variations of ability at different periods of the working day.

Only a few of Marsh's records have significance in themselves. The results are complicated by practice, which was

often allowed for on the basis of rather arbitrary assumptions. There were also only a few subjects who worked in any intensive way, and then usually for not over two weeks. But the results which are consistent and reliable in Marsh's work agree fairly closely with those of the present study.

The tests for speed and coördination of movement (striking squares, snapping, three-hole test, tapping) were similar in character to those here employed and the results are quite similar. "Most worthy of note . . . is the exceptional occurrence and decisiveness of the maximum tapping rate at 9:00–10:00 P.M. . . . . The maximum of accuracy comes earlier in the day than the maximum for speed . . . there is no doubt that it falls somewhere in the middle portion of the day" (p. 21). These tendencies are quite confirmed by the present results from *Tapping*, *Coördination* and *Typewriting*, in which mere speed showed a maximum at the end of the day, and speed with accuracy prescribed (*Coördination*) at the middle of the day.

In the case of strength Marsh found (using Cattell's ergograph and Collin's dynamometer) that "on the whole both subjects show most strength in the middle period (3:30 to 4:30 P.M. . . . . The curve of strength efficiency seems well established . . . a beginning minimum in early morning, a fairly rapid rise till 11:00, a level or slight decline till 1:00 P.M. (1 hr.), an increase to the maximum at 5:00 (1 hr.), thence a fall till bed time. . . . As a whole the figures show the same general course of efficiency for both sides of the body" (p. 31). These results are confirmed by work by Lombard, Patrizi, Harley, Christopher, Smedley, Oseretzkowsky and Kraepelin, and Storey.

In the cases of reaction-time, color naming, and opposites, in Marsh's work, the tests were so fragmentary and the subjects so few that the occasional superiority of successive trials is almost certainly only the result of practice, adaptation to the work, etc. Marsh remarks that in reaction time the superiority of the noon and late afternoon periods is apparent. In his simple sound-reactions this is the case, but in the discrimination reactions the superiority is anything but apparent.

In the case of memory Marsh's only reliable figures are those from one individual who memorized lists of German words at intervals of 1.5 hours, from 7:00 A.M. to 10:30 P.M. This is similar to the work of my own subjects on the intensive days, for the whole day (for 12 days) is said to have been spent in experimentation. Marsh was looking for variations within the day, and concludes (p. 53), "the total outcome as to memory must be considered a negation of the existence of a simple diurnal memory curve." As a matter of fact the records give a curve quite like that of all the mental tests in my own experiment, with clear signs of decreasing efficiency as the day goes on. The averages are as follows:

Hour	Time Taken
7:00 A.M.	371 sec.
8:30	390
10:00	386
11:30	434
1:30 P.M.	475
3:00	460
4:30	445
6:00	431
7:30	444
9:00	544
10:30	613

Many years ago Cattell studied the influence of fatigue on various form of reaction time by means of the records made by two observers who worked continuously for many hours. He wrote as follows: "In order further to investigate the effects of fatigue, I made extended series of experiments in which 1,950 reactions were made in succession, the observer reacting continuously from early in the morning until late into the night. Three series (78 reactions) were made with light, then three series (39 determinations but 78 mental processes) in which white light was distinguished and reacted on, then three series in which letters were seen and named, then two series in which associations were made, lastly three series of reactions on sound. This entire combination was repeated six times. The experiments were begun both days at 7:30 A.M. and were concluded in the case of *C* at 1:30 A.M. and in the case of *B* at 11:00 P.M., short pauses being made

for meals. One series of each variety was made the following morning and evening; in the case of *C* a further set of series the day after. . . . The first result to be noted from the table is the very slight effects of fatigue; in no case is the time lengthened more than a couple of hundredths of a second, and the mean variation is but little increased."[1]

Of the tests used by Cattell two were quite similar to tests used in the present experiment, viz., the discrimination reaction and the naming test (letters). His results for these two tests are as follows:

Time	Discrimination Reaction				Naming Letters			
	Obs. *B*		Obs. *C*		Obs. *B*		Obs. *C*	
	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.
7:30 A.M.	198	21	247	12	344	25	439	23
9:40	193	17	230	16	354	27	431	19
1:00 P.M.	207	17	248	21	337	34	448	23
2:50	207	25	237	19	348	26	458	20
6:55	232	18	253	21	356	31	476	26
8:50	218	19	249	16	381	30	469	23

In discrimination the fatigue by the end of the day (comparing last three trials with first three) is about 14 per cent. and 3 per cent., and in naming letters 7 per cent. and 8 per cent. with the two observers, about the same decrease in efficiency as is shown in these two tests in the present experiment, after the same amount of work.

In a recent study[2] Arai has presented an interesting picture of the course of efficiency during a day of continuous work. After considerable preliminary practice she reached an approximate practice level of performance in multiplying four-place numbers by four-place numbers, mentally. She then worked continuously (except for meal times) for 12 hours on each of four successive days, at such multiplication. During these four days there was, to be sure, increased speed due to practice, but this amounted to only about 20 per cent. of the initial speed by the end of the fourth day, thus averaging but 5 per cent. during any one day. The records are from

[1] *Mind*, 11, 1886, p. 537.

[2] 'Mental Fatigue,' T. C. Publications, 1912.

but one observer, but apparently from one who worked with unusual zeal and faithfulness. The curves of daily work, for each day, show pronounced and fairly regular diminution of efficiency, as the result of continued work. "Difficult and disagreeable continued work brings about a decrease in the efficiency of the function exercised. In the case of T.A. the time taken to do a certain number of examples is almost doubled during twelve hours of mental multiplication" (p. 114).

This is a much greater loss of efficiency than is shown in the present experiment, and the reasons are of course obvious. The mental multiplication of pairs of four-place numbers is a much more strenuous task than any of the tests applied to the present group of subjects. Further, in the present instance, the task was varied from time to time as the individual went through the various tests in succession. It is further true that these individuals travelled through the tests in squads of three each, so that after each test, each observer had a brief moment of rest while the other two made their records, and while the whole squad passed to an adjoining room for the next test.

Marsh insists strongly on the presence of a morning *inertness* which is the result of understimulation and protracted drowsiness. No such inertness is shown in my results, except in the motor tests. The inertness is probably not so much a nervous fact as it is a muscular mechanical one. In fact, as Marsh points out, "It is pronouncedly manifest in muscular abilities, especially strength, but seems less and less apparent and persistent as the mental field is entered."

The sharp difference between the effect of continued work on motor abilities, such as strength, speed, and coördination, and the effect on mental tests of perception, association, discrimination, memory, etc., is interesting. It undoubtedly falls in line with several other suggestions concerning the relations between the two kinds of work, and thereby throws considerable light on the general mechanisms of work. In the present investigation, as the day's work proceeds, motor processes gain and mental processes lose in

efficiency. Is there any significant relation between these two tendencies? The relation recalls Rivers's remark that he finds in himself and in others greater muscular power in moments of great mental fatigue or weariness. He concludes that the output of motor energy is ordinarily under the control of some inhibitory mechanism in the higher brain centers. During fatigue, after taking doses of certain drugs, etc., this inhibition seems to be weakened and there is for the time being an unrestrained overflow of motor innervation which quite exceeds the normal. Marsh also remarks (p. 79): "Where my subjects noted mental depression or even headache on their records, the figures rarely failed to show a high grade of muscular performance at the time."

The facts also fall well in line with Thorndike's suggestive remark that "we are fatigued not so much by what we do as by what we do not do." We may assume, rather roughly, perhaps, yet not without meaning, that as mental work proceeds, motor impulses and innervation tendencies which are of necessity suppressed during such work, become more and more insistent, and, according to the degree of insistence, interfere with the cortical interplay involved in the mental work. We would then have just the obverse of Rivers's explanation, for much the same set of facts. Motor processes would become more vigorous, not because of the removal of inhibitions, but, becoming more and more vigorous by cumulative impulsive tendencies, would themselves bring about the inhibitions which show themselves by way of diminished mental work.

Marsh remarks (p. 89): "The extent to which the inefficiency of the later part of the day is due to weariness and how far to real fatigue and nervousness is a hard question to solve." Other writers have also frequently suggested that the ordinary fatigue curves secured in experiments or incidentally shown in practical work, in no sense reveal an actual diminution of organic efficiency, but picture rather the effects of monotony, ennui, loss of interest, work, habits, etc. The great importance of these factors, both in the laboratory and in daily life, must of course be recognized. But it does

not at once follow that there is no genuine change in the nervous mechanism underlying the distribution and application of energy, as the result of continued work.

In the first place we should probably expect these general factors of weariness, etc., to affect all the tests in much the same way. There is at least no obvious reason why loss of interest should increase efficiency in *Tapping*, *Coördination*, and *Typewriting*, during the same periods in which it produces inefficiency in the whole range of more strictly mental performances. Nor is there obvious reason for believing that one test gains while another loses in interest. In fact all the obvious reasons are to the contrary. Nevertheless the various tests show characteristic differences in the effect, upon them, of continued work.

Further evidence that the loss of efficiency here portrayed is not due to such by-products as weariness, monotony, loss of interest, etc. is clearly shown, in the original experiments, by the influence of caffein on the drug days. The speed of performance, in *Opposites*, *Color Naming*, and *Adding*, for example, after the loss of efficiency has occurred, is strikingly quickened by appropriate amounts of the drug. It is not easy to see how the presence of caffein in the system modifies the interest of the tests (the change is not present after control doses).

That the form of the curves is not due to organic rhythms or diurnal factors is shown by the fact that the same curves are secured whether the work begins at 7:30, as in the preliminary experiment, or at 10:30 as in the case of the intensive experiment.

Most experimental attempts to demonstrate the presence or absence of genuine fatigue during the working day are invalidated because of the presence of improvement, either during the day or from day to day. In discussing his experiments on mental fatigue, Thorndike points out that incompetency does not come in proportion to the work done. "The decrease in energy does not have enough influence to outweigh the influence of practice."[1] This shows the neces-

[1] Psych. Rev., 7, 1900, 466, 547.

sity of using as subjects in fatigue experiments individuals who have already reached a practice limit in the tests used. And the limits should be genuine,—of such a sort that not only is there no gain during a given work period, but also no improvement as between one work period and the following one. This is indeed a difficult thing to accomplish with any great number of subjects, and in this respect the present experiments are particularly fortunate. This fact, coupled with the unusual zeal and constancy of incentive secured, lend high reliability to the data.

The conclusions, it should be noted, do not point to a general or special *fatigue factor*, nor to periodic variations of efficiency within the working day (except in coördination). They point rather to the presence of a complex work mechanism of such a sort that the influence of continued work upon more strictly mental processes differs characteristically from the effect on processes essentially muscular in character. This mechanism is one which stands over and above the transient inhibitions and reinforcements conditioned by interest, incentive, weariness, and similar affective or volitional factors.

Whatever the nature of this mechanism may be, it is such that processes essentially motor in character are facilitated and quickened by continuous work; processes involving coördination are first accelerated and then retarded again, approaching their original speed; processes essentially mental in character, when the work is done on a practice level of efficiency, show fairly uniform loss of efficiency, amounting to from 10 to 15 per cent., by the end of the day. Furthermore, the mechanism is of such a nature that it is acted upon in very definite ways, by the application of various drugs.

DISCUSSION

THE INHIBITORY FACTOR IN VOLUNTARY MOVEMENT. REPLY.

In the September number of the REVIEW Professor George V. N. Dearborn has discussed my article on "Voluntary Movement under Positive and Negative Instruction." His last paragraph begins: "None the less, it is interesting, to me at least, to observe that of Langfeld's five subjects that one (D) who had consistent kinesthetic imagery made the best record, a fact Langfeld fails to note in his conclusions, although obvious in the protocols." Since Professor Dearborn believes that "we are wasting not a little time and query over the matter of imagery in studying voluntary movement" the only interest he can have in this discovery is that it portrays extreme stupidity or intentional neglect upon my part. He has been, however, somewhat hasty in his criticism. I could not expect that he should be familiar with the Whipple tracing-board or that he could see from the rather indistinct lettering in the illustration of the board in Whipple's Manual, p. 120, that the larger numbers are at the wider end and that the stylus approaches zero as it moves down the groove. If, however, he had examined the tables and the protocols and the manner in which I treated the results, he would have discovered that the larger the figures the worse the results and not the reverse, as he has carelessly assumed. It is clear that subject D made the worst record.

H. S. LANGFELD.

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